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SECTION OF GEOLOGY AND MINERALOGY

MR. IRA H. CRAM, Assistant Chief Geologist, Pure Oil Company, President, American Association of Petroleum Geologists, Chicago, Ill.:

Imagination in Petroleum Geology

INTRODUCTION

It would be difficult to read a number of newspapers and magazines without noting some reference to geology. Many a layman who may have once thought of geologists as those peculiar fellows who spend their lives hunting dinosaur eggs in the Gobi desert or making useless observations in the crater of an active volcano has probably been surprised to learn that geologists are useful in the search for oil and other minerals, and even in modern warfare. The layman cannot be expected to have a good understanding of the many ways geology can be applied to the solution of practical problems, for the field of applied geology is broad beyond his, or, for that matter, any geologist's conception. Geologists are happy to be recognized as more useful citizens, and, in this wider recognition, they accept a challenge to be still more useful, to be alert to the existence of all problems dealing with the materials of the earth, to be ingenious enough to apply geology to the solution of these problems. Applied geology requires the exercise of every resource at the geologist's command. Above all, it requires imagination.

The petroleum industry is the largest employer of geologists and

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the record of applied geology in the industry is one that every geologist and every other scientist seeking to apply his science can study to advantage. Geologists have sold geology to the industry by applying their science successfully to the solution of a variety of problems many of which were once considered to be outside the sphere of geology. Today, geology permeates the producing branch of the petroleum industry and is as necessary to it as petroleum products are to our way of life. It is now obvious that the winning of oil from the earth is essentially a geologic enterprise and that the petroleum geologist is in a position to see more clearly than others in the industry the whole problem of making available adequate supplies of crude oil. In this position of opportunity and responsibility, the petroleum geologist can advance the application of geology as far as his resourcefulness and imagination will permit him.

Some Principles

There is no discovery formula. In 85 years of experience, geologists have developed but one fundamental principle governing the occurrence of oil deposits,—important deposits of oil are found only in traps in permeable rocks in geologic sections that are mainly sedimentary. The effectiveness of applying this broad principle to the discovery of new oil fields depends upon the geologist's imaginative use of his fund of geologic facts and theories, many of which are widely known. His fund of knowledge is increased daily by learning a certain number of the facts on the geology of oil deposits and the geology of the world, which are furnished at an indigestible rate of each new oil field, each well (whether dry or productive), and each geological and geophysical survey. His theories are subject to rapid change, because any new fact or group of new facts, or, for that matter, any old facts or group of old facts, when considered imaginatively and without prejudice, has a chance of upsetting several current theories, hypotheses and opinions and of leading to the discovery of one or more oil fields or provinces.

The petroleum geologist is both scientist and artist. He is a *scientist* when he works up the geologic picture or any part of the picture by ferreting out, assembling and coordinating original or other available data. In other words, he is a scientist when he is doing geology. But his job as a petroleum geologist has just begun when he completes his scientific work, for he has ahead of him the more important task of applying his geology,—of appraising the economic significance of his geo-

logical investigations. He is an *artist* when he applies successfully his science to the solution of any one of the many practical problems of the petroleum industry. The petroleum geologist's imagination is an important factor in his success in either capacity but especially in the capacity of artist.

Imagination in the Science of Petroleum Geology

I need not stress before a group composed largely of geologists the role of imagination in the science of geology. Anyone who completes a four-year course in geology must realize that the science of geology leaves much to the imagination. Quite certainly, no geologist can map the surface and subsurface of a sizable area without realizing that his imagination has supplied many missing links in the story. Ideally, the geologist's imagination should be suppressed until he has exhausted the possibilities of obtaining additional facts. However, certain considerations, largely economic, frequently make it desirable to attempt conclusions before exhausting the possibilities of obtaining additional facts.

Every bit of data pertaining to the earth, however obtained, is usable in the science of geology and is, in fact, geologic data. The petroleum geologist uses every scrap of data he can lay his hands on, employs all the principles of geology and, urged on by the economics of his problems, is vitally interested in improving the quality and quantity of geologic data. During the past 25 years, many instruments and techniques--plane table and alidade; core drill, magnetometer, torsion balance and pendulum; refraction seismograph, reflection seismograph and gravimeter; electric, gamma ray and mud logging devices; core analysis, fluoroscope, etc.- have been developed, that enable the geologist to obtain more precise earth measurements. All the instruments and techniques have their place in the sun. All furnish data of geologic importance, most of which were not obtainable 25 years ago. Geologists no longer have to imagine what the facts may be in certain instances, but they have to apply their imaginations to the solution of the new problems raised by the new data.

Many of these new geologic data are acquired by geophysical means and are frequently referred to as geophysical data. Unfortunately, too many petroleum geologists are content to turn over the geologic interpretation of these geologic data to the physicists who acquire the data. Geologists can squeeze a lot more geology out of these data acquired by geophysical means by taking the trouble to learn the

principles and, more important, to work with the data. As a matter of fact, geologists are old-timers in the use of geophysical instruments—the compass, the altimeter, the dip needle, the alidade, the binocular microscope, the petrographic microscope. By using the petrographic microscope, the geologist expands his understanding of the composition and structure of a piece of rock. By using the magnetometer, gravimeter and seismograph, he expands his understanding of the composition and structure of the earth. The difference is merely one of scale.

Geologists can and should expand their horizon by treating the data acquired by geophysical means as geologic data, worthy of being worked into the geologic picture, worthy of being subjected to analysis by the imaginative mind of a geologist. While I am urging geologists to delve more deeply into geophysics, I am at the same time urging them to delve as deeply into geochemistry, geobotany, geobiology or any other geoscience that is useful or that may be useful in the discovery of oil. Concurrently, I am urging the physicists, chemists, botanists, biologists and other scientists to study more geology. As geologists and other scientists attain a better understanding of each other's fields, the results of their combined efforts cannot fail to improve.

The intelligent use of the tools available to the petroleum geologist is a most important part of his job and requires perspective and imagination. In laying out his program, he has to make the best use of these tools, to choose the areas that merit attention first, to choose the proper tool or tools to use in each area. The choice of areas to work, either in reconnoissance or detail, is one of the most serious decisions the petroleum geologist has to make, and certainly few decisions require a greater exercise of good judgment and imagination. Keen competition makes it necessary to work the various areas in the order of their probable importance. No geologist or group of geologists has at his command sufficient resources to enable him to work, at the same time, all areas which may be considered prospective. Regardless of his resources he can dissipate them by the thoughtless use of them. The decision to explore a new area in the attempt to open up a new petroliferous province may be based upon little else than a few widely known geologic facts and a lot of imagination. This decision may be based on what amounts to a dream. In searching for a new pool in a petroliferous province, more facts are usually available, but again a choice must be made.

In the progress of the geological work which follows the decision

to explore an area, there is every opportunity to exercise imagination in the use of the tools. One example of the expert handling of a reflection seismograph crew in the Gulf Coast will serve to stress my point. In this certain area the shooting of profiles along the roads revealed a rather inconspicuous structural anomaly. Other companies had also shot along the same roads and one of them had drilled a dry hole on the anomaly. The geologist in charge could have passed up the structural anomaly as one of no importance and proceeded to search elsewhere for a more important structural feature. He saw in the picture, however, the suggestion of an important structural anomaly located between the lines of control along the roads. He therefore went to the trouble and expense of shooting lines through the rather inaccessible country between the roads and proved the existence of a sizable closed structure. Subsequent drilling of the closed structure resulted in the discovery of a good Gulf Coast oil field. The discovery might have been postponed indefinitely had this geologist not been imaginative in the use of an expensive tool.

Imagination in the Art of Petroleum Geology

It is the geologist's function to detail the geology of each prospect to the point that the drilling of a wildcat well becomes an economic venture. When the geologist determines that point, he has appraised the economic significance of his scientific endeavors and has assumed the role of artist. I do not wish to imply that the geologist's efforts up to this point are exclusively scientific. All along the line he has to consider the practical application of his work. Quite certainly there is more art than science in choosing the areas to work. In the capacity of artist or translator of geology into economics, the petroleum geologist is on his own. The machines he used to develop the geologic picture will not assist him to read into that picture the practical application of it. He has only his experience, knowledge, theories, reasoning powers and imagination to guide him. Fortunately, long experience in drawing conclusions from a miscellaneous assortment of data has sharpened his powers of imagination. Expressing this thought in different words, the petroleum geologist uses only one tool—his head—when he applies geology to the solution of practical problems. Since the most important practical problem is the discovery of oil, it follows that the future of oil discovery lies as much, if not more, in the minds of geologists as it does in the development of new prospecting tools, the im-

provement of old tools and drilling equipment, and the acquisition of new data. The burden on the minds of geologists promises to grow heavier rather than lighter, because the anticipated improvement in fact-finding devices will do well to keep pace with the increased difficulties of finding the huge quantities of oil necessary to our national welfare.

The libraries and files of companies and individuals are full of good geology indicating good prospects, a fair percentage of which will be found productive. That many of these prospects have not been considered as prospects by the many geologists who have studied these pieces of geology is of no great significance, for, in the absence of a discovery formula, the opinion of the majority does not necessarily approach the truth. Any geologist at any time may see one or more oil fields or new petroliferous provinces in these libraries or files by the not altogether simple process of tearing himself away from orthodox economic appraisal of the geology and adopting a fresh viewpoint. The more imaginative geologists can achieve this fresh viewpoint. The less imaginative achieve it with difficulty, for they are more inclined to search for oil in the light of the past—the old story of the generals fighting the last war. Orthodox economic appraisal of geologic features leads to mental condemnation of those geologic features that are unlike those upon which nearby or remote oil fields are located. Referring again to the broad principle that important deposits of oil are found only in traps in permeable rocks in geologic sections that are mainly sedimentary, there is little justification for mentally condemning a stratigraphic or structural condition affording a trap just because this trap does not meet the structural and stratigraphic specifications of nearby or remote oil fields. Only the drill can condemn a prospect.

Daring, imaginative interpretation of the oil possibilities of geologic features has led directly to the discovery of oil in the past, and it is well to relate an outstanding example—the Fitts field, Oklahoma. This field, discovered in 1933, is located in the Franks graben, a relatively small pie-shaped graben along the northeast flank of the Arbuckle Mountains. The geology of the area had been adequately described in a report published in 1924. Between the dates of publication and discovery any number of petroleum geologists had every opportunity to appraise correctly the oil possibilities of the prominent faulted anticline in Pennsylvania rocks within the graben. Most of these geologists in their search for oil were looking for geologic features resembling closely known Oklahoma oil fields, and the geology of the Franks

graben was different. A handful of geologists ignored all current theories and adopted the belief that the prominent faulted anticline in Pennsylvania beds, together with the westward convergence of the Pennsylvanian across the anticline, afforded a trap that was both structural and stratigraphic, meriting the drilling of a wildcat well. Those who felt that the prospect was too close to the mountains now can see Ordovician oil wells 4500 feet deep less than a mile from the Ordovician outcrop in the Arbuckle Mountains. The daring, imaginative, trail-blazing interpretation of the oil possibilities of a widely known anticline added over 100 million barrels of oil to the nation's reserve and, more important, it affected profoundly the thinking of hundreds of geologists. Prospects were no longer too close to the mountains, and geologists who had mentally condemned the areas around the mountains immediately became interested in them.

You have perhaps heard the opinion expressed that we have found and drilled most of the structural traps in the United States. It is more correct to say that we have found and drilled most of the structural traps that the majority of geologists *think* are good traps in the areas that the majority of geologists *think* are prospective. Fitts was not considered to be this sort of trap in this sort of area, and, today, as in 1933, there remain any number of structural traps that do not meet the specifications of the majority but which are nevertheless potential oil fields.

You have also heard that, in order to meet the demand for crude oil, geologists will have to devote more attention to the discovery of stratigraphic traps. One cannot quarrel with this statement. There are relatively few oil fields owing their existence to stratigraphic trapping alone, but there are many oil fields owing their existence to combined structural and stratigraphic trapping. In the future, as in the past, some stratigraphic oil fields will be discovered by random drilling, but, in the future, the petroleum geologist will have to develop more ideas that can be turned into stratigraphic oil fields by a reasonable amount of drilling. In order to develop these ideas the petroleum geologist has no alternative but to do more and better geology and to use more imagination in the appraisal of the economic significance of his geology.

A good example of an excellent oil field located on a combination stratigraphic and structural trap and discovered through the efforts of geologists is the East Coalinga Eocene pool in California. The wildcat well which proved to be the discovery well of this field was located on

the axis of the southeastward plunging Coalinga anticlinal nose, a well-known structural feature in California. Eight and one-half miles to the southeast of the well, on the same line of folding, a well in the Kettleman Hills field had revealed a thick section of Eocene sand. Seven miles to the northwest of the well and up-dip, the exposed Eocene section contained a poorly developed sand body. The well was located with the idea of penetrating a substantial sand section as far up-dip on the anticlinal nose as possible. As a matter of fact, the thick Eocene sand section in Kettleman Hills had been predicted from the general geologic considerations in advance of drilling, and leases had been purchased on the Coalinga nose as a play for Eocene production. Highly imaginative interpretation of a fund of accurate surface and subsurface data therefore preceded the discovery.

Conclusion

I do not pretend to have presented to you an exhaustive treatise on imagination in petroleum geology. A great deal more imagination goes into the discovery of most oil fields than is evident from a study of the literature, because authors seldom describe their mental processes. If the details were known, I am sure we should find that my attempt to portray the role of imagination in both the science and art of petroleum geology is woefully short of the mark. However, I hope to have cited enough examples of the economic importance of imaginative geology to clarify my conception that imagination permeates petroleum geology, just as petroleum geology permeates the producing branch of the petroleum industry.

It will take some new provinces full of oil fields as well as a great many new oil fields in known provinces to supply the heavy demand for domestic crude oil. Ideas, the product of imaginative minds, precede detailed geological investigations, steer the course of these investigations and control the economic interpretation of the results of the investigations. Without good ideas, nothing worth while is started, nothing worth while results. The quality of ideas is likely to be proportional to the available quantity of accurate geologic data, and the quality of the geologist's imagination. In the search for new provinces the geologic data are ordinarily scant; hence, the imagination is taxed heavily. Let us tax our imaginations, for, by taxing them, we develop them, and, by developing them, we generate more good ideas, resulting inevitably in more oil fields in both old and new areas.

SECTION OF BIOLOGY

October 9, 1944

Doctor S. R. Detwiler, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *On Factors Affecting Pigment Migration in the Retina.* (This lecture was illustrated by lantern slides)

It has long been known that illumination of the retina in many animals brings about a forward migration of the pigment in the epithelial pigment layer, contraction of the cones, and elongation of the rods. In dim light or "darkness," the inverse changes occur, viz., retraction of the pigment, elongation of the cones, and a shortening of the rods.

These changes have been termed "photomechanical," since they were first observed to occur in response to marked alterations in illumination. In teleost fishes, amphibians, and birds, the responses are prominent. In reptiles, they are greatly reduced, and in mammals, including man, their occurrence is questionable. It is generally regarded, however, that these responses are of great importance in adapting the retina to changes in illumination, particularly in the lower vertebrates (Walls, 1942). In man and other mammals, where these responses apparently do not take place, adaptation to changing illumination must be sought primarily in the physiology of the retinal photopigments in combination with pupillary responses.

Despite prolonged investigations (Arcy, 1915; Detwiler, 1943; Walls, 1942), the mechanism by which photomechanical shifts are brought about has never been fully elucidated. Neither have the various theories concerning the functional significance of these positional alterations met all the requirements of the inquisitive mind.

Photomechanical changes have been shown to be evoked by factors other than light and darkness. One of these factors is temperature. Arcy (1916a), working with teleost fishes, showed that at low (5° C.) and at high (25° C.) temperatures in the dark, the pigment expands, thus exhibiting a response characteristic of light adaptation. The cones, which typically lengthen in the dark, were found to shorten at low temperatures, and to elongate at high temperatures. The results of his experiments upon frogs were essentially in agreement with those

of Herzog (1905). Their general conclusion was that the pigment in the dark-adapted eyes undergoes striking expansion between 0°–14° C and 19°–33° C.; whereas, at intermediate temperatures, it is highly contracted. These limits apparently are too rigid, for it has since been shown (Detwiler and Lewis, 1926, and, more recently, in unpublished observations) that, after two hours exposure to darkness at temperatures as high as 24° C., the pigment is usually in the contracted state.

Recent experiments (Detwiler, 1944) have shown that excitatory stimuli, such as those occasioned by handling or clamping to a frog board, will cause extensive migration of the pigment in dark-adapted frogs. The migration is fairly rapid and can take place within twenty minutes. The excision of one eye (under faint red light) from a dark-adapted frog is invariably followed by expansion of the pigment in the opposite eye, following twenty minutes further exposure to darkness. This response is not necessarily due to the operation, for if a dark-adapted frog (2–3 hours in darkness) is clamped to a frog board, and kept in the dark for twenty additional minutes, the pigment is found to be in the extended condition.

It becomes clear from these and other results that, in any experiments involving the injection of various substances to test their effects upon photomechanical reactions, the experimenter must be extremely careful not to ascribe certain results to the substances injected, when the response may be due to the excitatory state produced by mere manipulation of the frog. Numerous cases are on hand to show that the injection in the dark of 1 cc. of frog Ringer's solution into the ventral lymph sac of dark-adapted frogs will cause the pigment to undergo expansion as extensive as that produced by excising an eye.

In order to investigate the possible nature of this so-called excitatory response, a number of experiments were performed in which the frog, after dark-adaptation, was anaesthetized. This was accomplished by injecting 1 cc. of a 10 per cent solution of paraldehyde into the ventral lymph sac. Usually, within three minutes, the frog became limp and showed no response to any peripheral stimulation. The left eye was then excised and the animal re-dark-adapted for 20 minutes. Despite the obvious blocking of all cutaneous impulses, the pigment in the right eye underwent migration in the dark comparable in extent to that obtained in unanaesthetized animals. The same results were ob-

tained when anaesthetizing doses of chloretone were used (1 cc. of a 1–100 solution).*

Whereas, the above observations show that functioning cutaneous nerves are not necessary for the pigmentary response, the question still remains as to what extent other nerves (e.g. autonomics), the action of hormones, or other chemical changes may be involved in the reactions In order to test these matters further, a series of experiments upon *Amblystoma* larvae was carried out as follows: A supernumerary optic vesicle was substituted for the ear vesicle in embryos in the early tail-bud stage of development. One group of embryos containing the supernumerary eye was then hypophysectomized. As a result, there became available for study two groups of animals. One group possessed the hypophysis, normal intact eyes, and heterotopic eyes lacking connection with the central nervous system. The other group, with normal and heterotopic eyes, lacked the hypophysis. The larvae were raised for 30 to 45 days, when individuals from both categories were subjected for four hours to light (60 watt lamp) or, for the same time, to total darkness It thus became possible to study the effects of light or darkness, with or without the presence of the hypophysis, upon normal eyes, as well as upon those devoid of nervous control. A microscopic study was made of the eyes from four light-adapted and four dark-adapted larvae with an intact pituitary gland, and of a similar number which had been hypophysectomized. The findings showed that the pigmentary response in the eyes of larvae lacking the hypophysis does not differ from that in the eyes of normal larvae. Furthermore, in the grafted eyes lacking nervous connections, the response is also of the same magnitude as in the normal eyes, regardless as to whether the hypophysis is present or absent. The results clearly point to the conclusion that, in this form, the reactions to light and darkness are autonomous, and are not subservient to nervous control nor to any influence on the part of the pituitary gland.

Laurens and Williams (1917) showed that, in heterotopic eyes of *Amblystoma tigrinum*, pigment migration and cone contraction under illumination took place as in the normal eyes, but to a greater extent Pigmentary responses of similar magnitude were shown to take place in the heterotopic eyes of *Rana fusca* larvae (Detwiler, 1929). The findings are in support of the generally known fact that, in certain

* Arey ('16a) noted that anaesthetizing dark-adapted catfish (*Ameiurus*), by immersing them in a 0 1 per cent solution of chloretone, did not prevent the migration of pigment when the animals were brought into the light

forms, limited positional changes can take place independently of the nervous system, despite evidence in support of the existence of a superimposed nervous (efferent) control (Arey, 1916a, 1916b).

That the pituitary gland plays no significant role in regulating photomechanical response is apparent, for the weight of evidence shows that after hypophysectomy, the retinal responses to light and darkness are unaltered. An exception to this is the work of Sverdlick (1942), who claims that the pigment in light-adapted hypophysectomized frogs undergoes moderate contraction. In my own experiments with hypophysectomized frogs, the responses to light and darkness under otherwise controlled conditions did not deviate from those exhibited by normal frogs subjected to similar experimental procedures.

In contrast to strong evidence that excision of the hypophysis does not effect the "light" and "dark" response of the retinal pigment, the results of experiments involving the injection of posterior lobe extracts (containing melanophore-stimulating hormone) are not only at variance, but are, in many instances, contradictory, as regards the effects on both dark-adapted and light-adapted frogs and toads. Matuo (1935) and Dubois-Poulsen (1937) obtained expansion in the dark following the injection of posterior lobe extract. One wonders whether the supposed influence of the hormonal substance injected, in many instances, is real. This query seems especially pertinent in view of the fact that the injection of frog Ringer's solution into the ventral lymph sac of dark-adapted frogs results in a rather striking expansion of the pigment, and is apparently the result of mechanical manipulation and subsequent excitation of the frog. This, however, would not account for the contraction of the pigment in illuminated eyes which several authors claimed to have observed after injection of posterior lobe extract (Drouet and Florentin, 1937).

That the injection of adrenalin causes a marked migration in dark-adapted eyes is generally agreed upon by those who have investigated this matter. In 1919, Arey (see also Arey and Jennings, 1943) showed that the injection of 2 cc. of a 1:100,000 solution would produce marked migration in the dark. He found also that extracts of the adrenal bodies of the frogs were also potent when injected. In view of my own experiments, it would appear that the marked migration following adrenalin injection is not all due to the substance injected, for some of the migration must in all instances be attributed to the sequelae of internal reactions which are initiated by the act of injection alone.

The completion of these reactions is apparently not dependent upon functional intactness of the cutaneous nerves, for in dark-adapted frogs, which are anaesthetized just prior to the injection of adrenalin chloride (1 cc. of 1:100,000), a marked migration of the pigment occurs in darkness. This may be maximal within 30 minutes.

Studnitz (1940) found that the injection of phosphoric acid into dark-adapted fishes caused the pigment and the cones to assume the "light" condition. He also cites evidence which indicates the migration of pigment caused by the injection of adrenalin chloride is due to its acidity and not to the hormonal base. He also claimed that the injection of alkali (KOH) into light-adapted fishes caused them to dark-adapt.

A few preliminary experiments by the author have been made to test the effects of acidosis and alkalosis upon the retinal responses. Injection of either ammonium chloride (1 cc. of a 5 per cent solution) or sodium bicarbonate (1 cc. of a 10 per cent solution) into dark-adapted frogs will bring about, in 30 minutes, migrations as extensive as those following the injection of adrenalin, but the experiments to date are too crude to justify any definite conclusions. More refined experiments employing acids and alkalies are under way. Experiments are also in progress to test the effects of parasympathetic sensitizers and paralysants upon the retinal responses to light and darkness. It is hoped that these and still other approaches will yield results which will be fruitful to a clearer understanding of the physiology of this fascinating, but baffling, problem.

Literature Cited

Arey, L. B.

1915. The occurrence and the significance of photomechanical changes in the vertebrate retina—an historical survey. J. Comp. Neur., **25**: 535–554.

1916a. The movements in the visual cells and retinal pigment of the lower vertebrates. J. Comp. Neur., **26**: 121–201.

1916b. The function of the efferent fibers of the optic nerve of fishes. J. Comp. Neur., **26**: 213–245.

1919. On the functional relations of the suprarenal gland and the retinal pigment. Anat. Rec., **16**: 138.

Arey, L. B. & W. K. Jennings

1943. The effects of darkness and temperature on the retinal pigment and visual cells of the frog's eye when transferred into the belly cavity. J. Comp. Neur., **79**: 487–499.

Detwiler, S. R.

1929. Some observations upon grafted eyes of frog larvae. Arch. f. Entw.-mech., **116**: 555–566.

1943. Vertebrate Photoreceptors. The Macmillan Company, New York.

1944. Excitation and retinal pigment migration in the frog. J. Comp. Neur., **81**: 137–145.

Detwiler, S. R. & R. Lewis

1926. Temperature and retinal pigment migration in the eyes of the frog. J. Comp. Neur., **41**: 153–169.

Drouet, P. & P. Florentin

1937. Revue Médicale de Nancy, **65**: 678 (cited from Sverdlick, 1942).

Dubois-Poulsen, A.

1937. Effects de l'extrait hypophysaire et de l'adrénaline sur les franges de l'epithélium pigmentaire de la rétine de la grenouille. Comptes Rendus de la Société de Biologie, **125**: 248–249.

Herzog, H.

1905. Experimentelle Untersuchung zur Physiologie der Bewegungsvorgänge in der Netzhaut. Arch. f. Anat. u. Physiol., Physiol. Abt., Heft. **5** u. **6**: 413–464.

Laurens, H. & J. W. Williams

1917. The photomechanical changes in the retina of normal and transplanted eyes of Amblystoma larvae. J. Exp. Zool., **23**: 71–83.

Matuo, K.

1935. Ueber die Wirkung des Hinterlappenhormons der Hypophyse auf die Pigmentwanderung der Netzhautpigmentzellen des Frosches. Okayama Igakkai-Zasshi, **47**: 2387.

von Studnitz, G.

1940. Physiologie des Sehens. Reis u. Wetzel. Leipzig.

Sverdlick, J.

1942. Influencia de la hipófisis y de la suprarenal sobre el pigmento retiniano del *Bufo arenarum* Hensel. Revista de la Soc. Argentina de Biologia, **18**: 207–214.

Walls, G. L.

1942. The vertebrate eye. Cranbrook Inst. Sci., Bull. **19**. Bloomfield Hills, Michigan.

SECTION OF PSYCHOLOGY

OCTOBER 16, 1944

DOCTOR DONALD MARQUIS, Director of the Office of Psychological Personnel, National Research Council, Washington, D. C.: *Psychology in the War.*

No abstract of this paper has been received.

SECTION OF ANTHROPOLOGY

October 23, 1944

Doctor Claude Lévi-Strauss, École Libre des Hautes Études, New York, N. Y.: *The Social and Psychological Aspect of Chieftainship in a Primitive Tribe: the Nambikuara of Northwestern Mato Grosso.*

Few anthropologists would admit today that human groups displaying an extreme primitiveness either in the field of material culture or that of social organization can teach us something about the early stages of the evolution of mankind. Primitiveness in one field often goes on a par with a great sophistication in another, as shown by the Australian refinements concerning kinship. Since these primitive peoples have their own history, it would be a serious mistake to think that it may be discounted because we know nothing of it. The partial similarities which archaeological remains allow us to infer between primitive societies and those of prehistoric man, while they remain sheer hypotheses, do not preclude the tremendous differences which may have existed in fields outside of the archaeologist's reach. The above considerations, which are only a few among many others, have led most anthropologists in recent years to consider each human group as a particular case which should be studied, analyzed and described from the point of view of its uniqueness, without any attempt to use the results for a better understanding of human nature.

However desirable this attitude may have been after the evolutionist orgies, and however fruitful the results obtained, there are many dangers in it which should raise increasing concern. Are we condemned, like new Danaids, to fill endlessly the sieve-like basket of anthropological science; in vain, pouring monographs over monographs without ever being able to collect a substance with a richer and denser value? Fortunately, primitive societies have not to be considered as illusory stages in the evolution of mankind to teach us a truth endowed with a general validity. The fact that they are (at least some of them and all of them in some respect) *simpler* societies than our own does not need to be taken as a proof of their archaism. They still throw light, if not on the history of mankind, at least on some basic forms of activity

which are to be found, always and everywhere, as prerequisites for the existence of human society.

The simpler organisms may provide a better field for the study of organic functions than those which exhibit the same functions, although under a more complex form. Simple human groups render the anthropologist the same kind of service without any need of surmising that they represent survivals of older types of organization. Now, to call upon the notion of function in the field of anthropological sciences is no discovery. This notion, first introduced by Durkheim in 1894,[1] has been only too much exploited since then, sometimes in the most abusive way. There are indeed functions of the social life as well as functions of the organic life. But neither in one domain nor in the other does everything correspond to, nor may it be justified by, its functional value. To state the opposite view could lead to only two results: either an anthropological come-back to eighteenth century Providentialism, where culture would play in relation to man the same utopian tutelary part which was attributed to nature by the author of *Paul et Virginie*;[2] or the reducing of the notion of function to a mere tautology—to say, for instance, that the function of the notched lapel on our coats is to gratify our esthetic feeling would be meaningless, since, here, obviously, the feeling results from the custom, and not the contrary. The custom has a history which explains its existence. It does not, under present circumstances, possess any function.

The preceding may appear to be a very ponderous introduction to an address dedicated by its title to the psychological aspects of chieftainship in a small Brazilian tribe. But I do not believe that the data which I am going to present, if considered only as data on chieftainship among a hitherto little known group, would honestly deserve one hour of attention. Similar facts have been recorded many times, either joined or separately. The particular interest offered by the Nambikuara is that they confront us with one of the simplest conceivable forms of social and political organization. Chiefs and chieftainship exist, among all human groups, under very different forms, but it would be vain to assign a special functional value to each of the modali-

[1] In "Les Règles de la Méthode Sociologique": "The function is the correspondence between the considered fact and the general needs of the social organism." p. 117.

[2] In his "Etudes de la Nature" (1784) **Bernardin de Saint Pierre** suggested that Nature devised melon ribs to make the fruit easier to divide on the family table, and that it made fleas black so that they could more easily be caught on white skin.

ties down to their smallest details. There is, undoubtedly, a function in chieftainship. This can, however, be reached only through analysis as the underlying principle of the institution. In other words, the differing structure of the digestive organs in man, ox, fish and clam do not point toward different functions of the digestive system. The function is always and everywhere the same, and can be better studied, and more fully understood where it exists under a simple form—for instance, in a mollusc. Similarly, and as Professor Lowie once wrote, if anthropology is to be considered as a scientific study, its subject matter cannot be individual cultures, but culture taken as a whole; the rôle of individual cultures being to offer, according to their own characteristics, special angles from which the basic functions of culture, although universal in application, can be more easily reached.

This will perhaps help us to eliminate preliminary questions which otherwise could have proved very difficult. Anthropologists in South America and elsewhere have been eagerly debating the question of whether these South American tribes—nomadic, relying mostly on collecting and gathering, with little or no agriculture, little or no pottery, and, in some cases, with no dwelling other than crude shelters— should be considered as truly primitive and as having preserved their exceptionally low cultural level through tarriance, or whether they did not previously possess a higher type of social and material organization and have regressed to a pseudo-archaism under unfavorable circumstances. The Nambikuara are one of those tribes which, along with the Siriono, on the other side of the Guaporé valley, the Cayapo, Bororo, Karaja of central Brazil, the so-called *Gé* of Central and Eastern Brazil, and some others, together form a kernel of primitiveness surrounded, in the West, by the higher tribes of the upper Amazon, the Bolivian plain and the Chaco, and from the Oronoco's to the La Plata's estuaries, by a coastal strip inhabited mostly by the Arawak, Carib and Tupi-Guarani linguistic families. An independent linguistic stock divided into several dialects, the Nambikuara seem to display one of the more backward cultures in South America. At least, some of their bands do not build huts and are wholly ignorant of pottery and, even among the others, these two arts are exceedingly poor. There is no weaving, except for the narrow arm and leg bands which are made of cotton; no dress whatsoever, either for the men or for the women; no sleeping contrivances, such as hammocks or platforms; the natives being used to sleeping on the bare ground without the protection of blankets, mats or hides.

Gardening exists only during the rainy season and does not free the Nambikuara from wandering during the seven months of the dry season, looking for wild roots, fruits and seeds, small animals such as lizards, snakes, bats, spiders and grasshoppers and, generally speaking, anything which may prevent them from starving. As a matter of fact, their geographical surroundings, which are located in the northwestern part of the state of Mato Grosso and include the headwaters of the Tapajoz, Rio Roosevelt and Rio Gi-Parana, consist of a desolated savanna with few vegetal resources and still less game.

Had I approached my subject from a point of view other than the one outlined above, I could not have avoided a long discussion in South American cultural history, aimed at clearing up this apparent primitiveness, on the question as to whether the survival of early conditions of life in South America is genuine or whether we should consider it as a more recent—although undoubtedly pre-columbian—result of culture clashes and processes of acculturation. Whatever the answer may be, it cannot substantially change our problem: whether tarriant or recessive, the Nambikuara society functions, in the present, as one of the simplest forms of human society to be conceived. We shall not seek information from the particular history which kept them in their exceptionally crude organization or brought them back to it. We shall only look at the experiment in social anthropology which they now enact under our very eyes.

This holds especially true in respect to their social and political life. For if we do not know what was the material culture of the Nambikuara forty years ago (they were discovered only in 1907), we do know that their numbers became tremendously reduced after their contact with white civilization. General (then Colonel) Candido Mariano da Silva Rondon, who discovered and studied them, first stated that their number was about 20,000. This was around 1915. I take this figure as greatly exaggerated, but even if reduced by one half, it considerably exceeds the present number which is hardly more than 2,000. Epidemics have taken care of the difference. What does this mean, from the point of view of our study? During the dry season, the Nambikuara live in nomadic bands, each one under the leadership of a chief, who, during the sedentary life of the rainy months, may be either a village chief or a person of position. General Rondon wrote that, at the time he was exploring the country, it was not rare to see bands averaging two or three hundred individuals. Now, sixty or seventy

people are seldom met together, the average size of the bands being twenty individuals, women and children included. This demographic collapse cannot possibly have taken place without affecting the structure of the band. But here, too, we do not need to concern ourselves with such questions as the type of political organization in earlier times. It is probably more difficult to understand Nambikuara sociology now than it was thirty years ago. Perhaps, on the contrary, the much reduced Nambikuara band offers, better than in the past, a privileged field for a study in social anthropology. My contention is that, precisely on account of its extreme impoverishment, Nambikuara political structure lays bare some basic functions which may remain hidden in more complex and elaborate systems of government.

Each year, at the end of the rainy season, that is, in April or in early May, the semi-permanent dwellings laid in the vicinity of the gallery-forest where the gardens are cleared and tilled, are abandoned and the population splits into several bands formed on a free choice basis. Each band includes from two to about ten families usually tied by kinship. This may be misleading when a band is met, for one easily gets the impression that it is formed as an extensive family. It does not take long to discover, however, that the kinship tie between two families belonging to separate bands may be as close, and eventually closer, than between two families inside the same band. The Nambikuara have a simple kinship system based on cross-cousin marriage and the subsequent dichotomy between "cross" and "parallel" in every generation. Therefore, all the men in one generation are either "brothers" or "brothers-in-law," and men and women are to one another either siblings (true or classificatory) or spouses (true or classificatory). Similarly, children are, in relation to the adults, either sons and daughters (true or classificatory) or nephews and nieces, which is the same as actual or potential children-in-law.[3] As a result, there is no great choice of terms to express kinship, and this explains why kinship inside the band may appear closer than it actually is, and kinship between people belonging to different bands more remote than shown by genealogies. Furthermore, a bilateral cross-cousin marriage system functioning in a relatively small tribe must produce a progressive narrowing, and even a multiplication, of the kinship ties between any two individuals. This is a supplementary reason preventing family

[3] C. Lévi-Strauss. "The Social Use of Kinship Terms among Brazilian Indians." American Anthropologist, 45 (3) 1943.

relationship from becoming really operative in the constitution of the band. It can be said that, inside the band as well as between the different bands which are the offspring of the same temporary village, everybody is everybody's kin, in pretty much the same fashion.

Why then the splitting-up process? Two different considerations must be brought forth to answer this question. From an economic point of view, the scarcity of wild food resources and the subsequent high square-mileage needed to feed one individual during the nomadic period make the division into small bands almost compulsory. The real question is not why there is a division but rather on what basis it takes place. I have said that this is done by free choice, but this freedom is not arbitrary. There are, in the initial group, several men acknowledged as leaders (who likely acquired this reputation from their behavior during the nomadic life) and who make the relatively stable nuclei around which the different aggregates center. The importance, as well as the permanence of the aggregate through successive years, depend largely upon the ability of each of these leaders to keep his rank and eventually to improve it. Thus, it may be said that leadership does not exist as a result of the band's needs, but, instead, that the band receives its shape, its size, and even its origin, from the potential leader who antedates it.

There is, however, a continuous function of leadership, although not permanently assumed by the same individual. Among the Nambikuara, chieftainship is not hereditary. When a chief grows old, or is taken ill, and when he does not feel able to fulfill his heavy duty any more, he himself designates his successor. "This one—this one will be the chief . . ." he says. It seems likely that this autocratic power to insure one's own succession is more apparent than real. We shall emphasize later on the small amount of authority enjoyed by the chief and, in this case as in many others, the final decision is probably preceded by a careful survey of public opinion, the designated heir being, at the same time, the one with the greater support from the members of the band. The appointment of the new chief is not only limited by the wishes or disapproval of the band; it needs also to correspond to the plans of the individual to be chosen. Not seldom, does the offer of leadership meet with a vehement refusal: "I don't want to be the chief." Then a new choice must be made. As a matter of fact, chieftainship does not seem to be coveted by many people, and the general attitude of the different chiefs I happened to know was less to brag about their

importance and authority than to complain of their many duties and heavy responsibilities. What, then, are the privileges of the chief, and what are his obligations?

When, about 1560, the great French moralist of the sixteenth century, Montaigne, met in Rouen with three Brazilian Indians brought there by some navigator, he asked one of them what were the privileges of the chief (Montaigne said, "the King") in his country; and the native, himself a chief, answered: "To walk ahead on the warpath." Montaigne related this story in a famous chapter of the *Essays* where he wondered a great deal about this proud definition[4]; but it was a greater wonder to me when, almost four centuries later, putting the same question to my informants I was given the same answer. Civilized countries are certainly not accustomed to such constancy in the field of political philosophy! Striking as it may be, this answer is less significant than the name by which the chief is designated in the Nambikuara language. *Uilikande*, the native word for chief, seems to mean "the one who unites" or "the one who joins together." This etymology suggests that the native mind is fully conscious of this extremely important phenomenon which I have pointed out from the beginning, namely, that the leader appears as the cause of the group's willingness to aggregate rather than as the result of the need for a central authority felt by a group already constituted.

Personal prestige and the ability to inspire confidence are thus the foundations of leadership in Nambikuara society. As a matter of fact, both are necessary in the man who will become the guide of this adventurous experiment: the nomadic life of the dry season. For six or seven months, the chief will be entirely responsible for the management of his band. It is he who orders the start of the wandering period, selects the routes, chooses the stopping points and the duration of the stay at each of them, whether a few days or several weeks. He also orders and organizes the hunting, fishing, collecting and gathering expeditions, and determines the conduct of the band in relation to neighboring groups. When the band's chief is, at the same time, a village chief (taking the word village with the restricted meaning of semi-permanent dwelling for the rainy season), his duties do not stop there. He will determine the moment when, and the place where, the group will settle; he will also direct the gardening and decide what plants

[4] **Michel de Montaigne.** "Des Cannibales." Essais, Livre I, **XXXI** (End of the chapter).

are to be cultivated; and, generally speaking, he will organize the occupations according to the seasons' needs and possibilities.

These rather versatile duties, it should be pointed out from the start, are not facilitated by any fixed power or recognized authority. Consent is at the origin of leadership, and consent, too, furnishes the only measure of its legitimacy. Disorderly conduct (according to the native standards) and unwillingness to work on the part of one or two discontented individuals may seriously jeopardize the chief's program and the welfare of his small group. In this eventuality, however, the chief has no coercivitive power at his disposal. The eviction of the bad people can take place only in so far as the chief is able to make public feeling coincide with his own opinion. Thus, he must continuously display a skill belonging more to the politician trying to keep hold of his fluctuating majority than to an over-powering ruler. Furthermore, he does not only need to keep his group together. Although the band lives practically alone and by itself during the nomadic period, the existence of the other bands is not forgotten. It is not enough to do well; the chief must try—and his people count on him for that—to do better than the others.

No social structure is weaker and more fragile than the Nambikuara band. If the chief's authority appears too exacting, if he keeps too many women for himself (I shall later analyze the special features of the chief's polygamy), or if he does not satisfactorily solve the food problem in times of scarcity, discontent will very likely appear. Then, individuals, or families, will separate from the group and join another band believed to be better managed. For instance, this band may get better fare from the discovery of new hunting or gathering emplacements; or it may have become richer in ornaments or implements, through trade with neighboring groups, or more powerful as a result of a successful war expedition. The day will come when the chief finds himself heading a group too small to face the problems of daily life, and to protect his women from the covetousness of other bands. In such cases, he will have no alternative but to give up his command and to rally, together with his last followers, a happier faction. Therefore, Nambikuara social structure appears continuously on the move. The bands take shape, then disorganize, they increase and they vanish. Within a few months, sometimes, their composition, number and distribution cannot be recognized. Political intrigues within the same band and conflicts between bands impose their rhythm upon these fluc-

tuations, and the ascent or decline of individuals and groups follow each other in a rather surprising manner.

How will the chief be able to overcome these difficulties? The first instrumental force of his power lies in his generosity. Generosity —an all important feature of chieftainship among most primitive peoples, especially in America—plays an outstanding part even on those crude cultural levels where worldly goods are limited to the most primitive weapons and tools, coarse ornaments made of feathers, shells and bones, and raw materials, such as lumps of rosin and wax, hanks of fiber and splinters of bamboo for arrow-making. There cannot be great economic distinctions between families each of which can pack all of its belongings in the baskets carried along by the women during the long travels of the dry season. But, although the chief does not seem to fare better, in this respect, than the others, he must always have at hand surpluses of food, tools, weapons, ornaments which, while being small indeed, acquire great value because of the scarcity which is the prevalent condition. When an individual, a family or the band itself needs or covets something, the chief is called upon to secure the desired article. Generosity is the quality, much speculated on, which is expected of a new chief. Generosity is the string constantly struck which makes the general consent to one's leadership sound clear or out of tune. There is little doubt that, in this respect, the chief's ability to give is exploited to the utmost. Band chiefs used to be my best informants, and, well aware of their difficult position, I liked to reward them liberally; but I seldom saw one of my many gifts remain in their hands for more than a few days. Each time I took leave of a band, after a few weeks or a few months, its members had time to become the happy hoarders of axes, knives, beads, and so on. As a rule, however, the chief was exactly as poor as at my first arrival. Everything he had received from me (and this was considerably more than the average) had already been squeezed out of him. This collective greediness not seldom drives the chief to an almost desperate position; then the refusal to give plays about the same part, in this primitive democracy, as the threat to resign followed by a vote of confidence in a modern parliament. When a chief reaches the point where he must say: "To give away is over! To be generous is over! Let another be generous in my place!", he must, indeed, be sure of his power and prestige, for his rule is undergoing its severest crisis.

Ingenuity is but the intellectual form of generosity. A great deal

of skill and initiative are the prerequisites of a good leader. It is he who makes the arrow-poison, although the preparation of *curare* among the Nambikuara is a purely profane activity surrounded by no ceremonial taboos or magic prescriptions. It is he, also, who makes the rubber ball used in the head-ball games which are played occasionally. The chief must be a good singer and dancer, a merrymaker always ready to cheer up the band and to brighten the dullness of daily life. This could easily lead to shamanism; and, in some cases, I have met with chiefs who were at the same time healers and trance addicts. Mystical life, however, is kept in the background among the Nambikuara, and, wherever they exist, magical functions are only secondary attributes of the leader. More often chieftainship and sorcery are divided between two different individuals. In this respect, there is a strong difference between the Nambikuara and their northwestern neighbors the Tupi-Kawahib among whom the chief is, first of all, a shaman, usually a psychotic addicted to dreams, visions, trances and impersonations.

But although they are oriented in a more positive direction, the Nambikuara chief's skill and ingenuity are none the less amazing. He must have a perfect knowledge of the territories haunted by his and other groups, be familiar with the hunting grounds, the location of fruit-bearing trees and the time of their ripening, have some idea of the itineraries followed by other bands, whether hostile or friendly. Therefore, he must travel more, and more quickly, than his people, have a good memory, and sometimes gamble his prestige on hazardous contacts with foreign and dangerous people. He is constantly engaged in some task of reconnoitering and exploring, and seems to flutter around his band rather than lead it.

Except for one or two men without actual power, but eager to cooperate and to receive occasional rewards, the passivity of the band makes a strong contrast with its dynamic leader. It seems as if the band, having relinquished certain advantages to the chief, were in exchange relying entirely upon him for its interests and safety. I received a particularly striking demonstration of this under rather strange circumstances. After several weeks' discussion, I had obtained from a chief the favor of taking me, together with a few companions and some animals loaded with presents, to the semi-permanent dwellings of his band which were uninhabited at that time. This was a chance for me to penetrate more deeply into the unexplored Nambikuara territory

and to meet groups too shy to venture forth on the outer fringe. The native band and my own group set out together on a journey supposed to be short; but, because of the animals I had taken, the chief had decided that the usual route through a dense forest could not be used. He led us through the open country, lost his way several times, and we did not reach our destination on the scheduled day. Supplies were exhausted and no game was in sight. The not unfamiliar prospect of a foodless day fell gloomily upon the natives. But, this time, it was the chief's responsibility. The whole project was his own, as well as the attempt to find an easier route. So, instead of trying to discover food, the hungry natives simply lay down in the shadow of the brush and waited for their leader to take them out of this most unpleasant situation. He did not wait or discuss; but, taking the incident as a matter of course, he simply left the camp accompanied by one of his wives. At the camp, the day was spent sleeping, gossiping and complaining. There was no lunch or dinner. But, late at dusk, the chief and his wife reappeared, both heavily laden with baskets filled to the brim. They had hunted grasshoppers the entire day, and, although the expression "to eat grasshoppers" has approximately the same meaning in Nambikuara as the French *manger de la vache enragée*,[5] this food was enthusiastically received, shared and consumed, amidst restored good humor. The following morning, everybody armed himself or herself with a leafless twig and went grasshopper-hunting.

I have several times referred to the chief's wives. Polygamy, which is practically the chief's privilege, brings him a moral and sentimental reward for his heavy duties together with the practical means of fulfilling them. In the Nambikuara band, apart from rare exceptions, only the chief and the sorcerer (when these functions are divided between two individuals) may have several wives. The chief's polygamy, however, presents special features. It does not constitute a plural marriage but rather a monogamous marriage to which relations of a different nature are added. I have already mentioned the fact that cross-cousin marriage is the usual pattern among the Nambikuara. Another type of marriage also exists, between a man and a woman belonging to the generation following his own, either a wife's "daughter" (true or classificatory) or a sister's niece. Both forms are not uncommon in South America and, together or separately, they have been re-

[5]Closest English equivalent: "to have a rough time of it, to go through the mill."

corded among many tribes. Now, what do we find in the chief's case? There is first a monogamous marriage of the cross-cousin type, that is, where the wife belongs to the same generation as her husband. This first wife plays the same part as the monogamous wife in ordinary marriages. She follows the sexual pattern of the division of labor, taking care of the children, doing the cooking, and collecting and gathering wild food. To this marriage are added one or several unions, which, technically, are true marriages, but of a different type. Usually, the secondary wives belong to a younger generation. The first wife calls them daughters or nieces. Besides, they do not follow the sexual pattern of the division of labor, but share indifferently in men's or women's activities. At the camp, they disdain domestic tasks and remain idle, either playing with the children to whose generation they belong or flirting with their husband, while the first wife keeps busy with the food and the fire. On the contrary, when the chief leaves on an exploration, a hunt, or some other manly task, they will accompany him and bring him their moral and physical help. These somewhat "tomboy" girls, elected by the chief from among the prettiest and healthiest of the group, are to him rather "girl-friends" than spouses. They live on the basis of an amorous friendship which contrasts strongly with the more conjugal atmosphere of the first marriage.

This system exerts a tremendous influence upon the whole life of the group. The periodical withdrawal by the chief of young women from the regular cycle of marriages creates a permanent unbalance within the group, between the number of boys and girls of marriageable age. Young men are the chief victims of that situation and must either remain bachelors for several years or marry widows or old women discarded by their husbands. Thus, the right to plural marriages represents a concession of considerable importance made by the group to its leader. What does it mean from the latter's point of view? There is little doubt that access to young and pretty girls brings him a much appreciated gratification, not so much from the physical side (as the Nambikuara share in the quiet dispositions of most South American tribes), as from the psychological and sentimental one. But, above all, plural marriage, together with its distinctive features, constitutes the technical means and the functional device placed at the chief's disposal by the group to enable him to carry out his exacting duties. Left by himself, he could hardly do more than the others. His secondary wives, freed by their special status from the customary

liabilities of their sex, are his helpers, comforters and assistants. They are, at the same time, leadership's prize and instrument. Can it be said, from the native point of view, that the prize is worth the trouble? To answer that question, I shall now have to consider the problem from a broader angle, namely, what does this elementary social structure, the Nambikuara band, teach us about leadership, its basis and its function?

There is a first point which does not require great elaboration. Nambikuara data contribute, with many others, to destroy the belief originated by early anthropologists, and temporarily revived by psychoanalysis, that the primitive chief could find his prototype in a symbolical father, and that the simpler forms of the State could progressively have grown out of the family. We have found at the root of the crudest forms of chieftainship a decisive step, which introduced something entirely new in respect to biological relations—and this step consists of *consent.* Consent, we have seen, is at the same time the origin and the limit of leadership. Unilateral relations such as right of age, autocratic power, or others, may appear in groups having an already complex structure. In simple forms of social organization, such as the one I have tried to describe, they are inconceivable. Here, on the contrary, the relationship between the chief and the group can be seen as a perpetual process of arbitration where the chief's talents and authority on the one hand and the group's size, cohesion and willingness, on the other, constantly react on and influence each other. If I had the time, and if it were not so far removed from my topic, I would have liked to show what considerable support modern anthropological observations bring, in this respect, to the analysis of the eighteenth century social philosophers. I am well aware of the fact that Rousseau's "social contract," which is the step by which individuals resign their autonomy in favor of the General Will, is entirely different from the nearly contractual relations existing between the chief and his followers. It remains true, however, that Rousseau and his contemporaries displayed a keen sociological feeling when they understood that cultural attitudes and elements such as "contract" and "consent" are not the result of secondary processes, as claimed by their opponents; they are culture's raw materials, and it is impossible to conceive a political or social organization in which they would not already be present. If I understand correctly, the recent analysis, by modern American

anthropologists, of the state-growth significance of military societies among the Plains Indians leads to exactly the same conclusion.[6]

My second point is but an exemplification of the first: consent is the psychological basis of leadership, but in daily life it expresses itself in, and is measured by, a game of give-and-take played by the chief and his followers, and which brings forth, as a basic attribute of leadership, the notion of reciprocity. The chief has power, but he must be generous. He has duties, but he is entitled to several wives. Between him and the group, there is a perpetual balance of prestations, privileges, services and obligations. The notion of reciprocity, originated by Marcel Mauss, was brilliantly analyzed by Malinowski in his "Crime and Custom in Savage Society." In respect to leadership, he says: "The claims of chief over commoners, husband over wife, parent over child and vice versa are not exercised arbitrarily and one-sidedly, but according to definite rules, and arranged into well-balanced chains of reciprocal services."[7] This statement needs somewhat to be completed. Malinowski is right when he points out that the chief-commoners' relationship, as every relationship in primitive society, is based on reciprocity. In the first case, however, the reciprocity is not of the same type as in the others. In any human society, whether primitive or civilized, two different cycles of reciprocity are constantly at work: first, the chain of individual prestations linking the isolated members of the group; and, next, a relation of reciprocity binding the group considered as group (not as a collection of individuals) and its ruler. In the case we have studied, this is well illustrated by the rules of marriage. Taken in its broadest sense, the incest prohibition means that everybody in the group is obliged to deliver his sister or daughter to an individual; and, conversely, is entitled to receive his wife from the latter (whether from the same man, as in exchange-marriage, or from a different one). Thus, a continuous chain of reciprocal prestations is directly or indirectly set up between all the collective or individual members of the group.[8] This may be called qualitative reciprocity; but incest prohibition also provides the basis for a quantita-

[6] **R. H. Lowie.** 1927. The Origin of the State: 76–107; New York. **K. N. Llewellyn & E. A. Hoebel.** 1941. The Cheyenne Way. Part II, ch. 5. University of Oklahoma Press.

[7] **B. Malinowski.** 1940. Crime and Custom in Savage Society: 46. New York. (Third Printing).

[8] See the late **F. E. Williams'** remarkable analysis in "Papuans of the Trans-Fly": 167–169. Clarendon Press. Oxford. 1936.

tive reciprocity. We may consider it as a "freezing" measure, which, while it forbids the appropriation of women who are at one's natural disposal, prepares the formulation of marriage rules allowing every man to get a wife. Therefore, a close relationship exists in a given society between the forbidden degrees and the extent to which polygamy is allowed. How does the preceding apply to the Nambikuara? If they had cross-cousin marriage associated exclusively with monogamy, there would be a perfectly simple system of reciprocity (from the individual's point of view) both qualitative and quantitative. This theoretical formula is, however, upset by the chief's privilege to polygamy. The withholding of the simpler rule, in favor of the chief, creates for each individual an element of insecurity which would otherwise not exist. Let us state this in other terms: the granting of polygamous privilege to the chief means that the group has exchanged *individual elements of security* resulting from the monogamous rule for *collective security* provided by leadership. Each man receives a wife from another man, but the chief receives several wives from the group. In exchange, he offers to guarantee against need and danger, not to the individuals whose sisters or daughters he marries; not to those who will be deprived of a spouse by his polygamous right; but to the group, taken as a whole. For it is the group, taken as a whole, which has withheld the common law in his favor. The preceding considerations may have some bearing upon the theory of plural marriage; but, most of all, they remind us that the interpretation of the State, conceived as a security system, recently revived by discussions about a national insurance policy (such as the Beveridge plan and others), is not a modern development. It is a return to the basic nature of social and political organization.

So much for the group's point of view on leadership. What about the chief's own attitude in relation to his function? What is his incentive in assuming duties of which I have given a not too favorable account? We saw that the Nambikuara band leader has a tiresome and exacting role; that he must exert himself without pause to maintain his position. What is more, if he does not constantly improve it, he runs the risk of losing what he has taken months or years to achieve. This explains why many men, as I have already said, shun leadership. But why do others accept and even seek it? It is always difficult to appraise psychological motives; and the task is almost impossible when a culture totally alien to our own is considered. I venture to say,

however, that the polygamous privilege, highly valued as it may be from the point of view of sexual gratification, sentimental appeal and social prestige, would not suffice to determine a leader's vocation. Plural marriage is but a technical prerequisite of chieftainship; its individual value can only be residual. There must be something more; and, going over the moral and psychological features of the Nambikuara chiefs I knew, and trying to hold on to those fugitive and irreplaceable glimpses at their intimate selves (of which no scientific approach may certify the accuracy, but which gain, from a deep feeling of friendship and human communication, some sort of intuitive value), I feel imperiously led to this answer: there are chiefs because there are, in any human group, men who, unlike most of their companions, enjoy prestige for its own sake, feel a strong appeal to responsibility, and to whom the burden of public affairs brings its own reward. These individual differences are certainly emphasized and "played up" by the different cultures, and to unequal degrees. But their clear-cut existence in a society as little competitive as the Nambikuara strongly suggests to my mind that their origin itself is not cultural. They are rather part of those psychological raw materials out of which any given culture is made. Men are not all alike; and, in primitive societies, believed by early anthropologists to be overwhelmed by the crushing power of custom, these individual differences are as keenly perceived and worked out as in our so-called "individualistic" civilization.

It is remarkable how far the practical experience of colonial administrators has outgrown, in relation to the previous considerations, anthropologists' theoretical studies. During the past twenty years, Lowie's pessimistic appraisal of anthropological work in the field of political institutions[9] has certainly not lost its value. We have much to learn from the scientifically untrained who deal with native institutions. I shall not here record Lyautey's testimony without reservation: "In every society, there is a leading class born for leadership and without which nothing can be accomplished."[10] What may be true for the simpler structures cannot be considered equally valid when considering the complex ones, where the function of leadership does not manifest itself any more in a "pure" state. But let us listen to Eboué who passed away a few months ago. Himself a full-blooded negro, he

[9] At the beginning of Chapter XIII of "Primitive Society."

[10] Quoted in: Governor-General Felix Eboué's Memorandum on "Native Policy," issued on November 8,1942.

wrote the following when he was Governor-General of French Equatorial Africa in special relation to those nomadic tribes which, as he put it, "live under a regime of organized anarchy." I quote: "Who is to be chief? I shall not answer, as was the custom in Athens, 'the best.' There is no best chief, there is just a chief;" and further; "the chief is not interchangeable . . . the chief pre-exists."[11] This is precisely what was suggested to us from the start of our analysis of Nambikuara society.

In conclusion, I submit that, when developing the study of political institutions, anthropologists will have to pay more and more attention to the idea of "natural leadership." I am well aware that this expression is almost contradictory. There is no possible form of leadership which does not receive its shape and specification inside of a given cultural context. But this expression can be taken as a borderline case, or as a limit—as say the mathematicians. While the limit can never be reached, simple social structures give us, in the order of their simplicity, an even closer approximation of it. In such studies, we may accordingly foresee a privileged field for close cooperative work between anthropology and individual psychology.

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[11] *Ibid.*

SECTION OF PHYSICS AND CHEMISTRY

OCTOBER 27 AND 28, 1944

Conference on *"The Diffusion of Electrolytes and Macromolecules in Solution."*

The Section of Physics and Chemistry held a Conference on "The Diffusion of Electrolytes and Macromolecules in Solution," as the first in the series for the Academic Year 1944–1945. Doctor Lewis G. Longsworth, Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introduction to the Conference—A Historical Survey," by Lewis G. Longsworth.

"Theories and Problems of Liquid Diffusion," by Lars Onsager, Yale University.

"A Conductance Method for the Determination of the Diffusion Coefficients of Electrolytes," by Herbert S. Harned and Douglas M. French, Yale University.

"The Diaphragm Cell Method of Measuring Diffusion," by A. R. Gordon, University of Toronto.

"Diffusion Constant Measurement in Theory and Practice," by Edward M. Bevilacqua, Ellen B. Bevilacqua and Margaret M. Bender, University of Wisconsin.

"The Effects of Concentration and Polydispersity on the Diffusion Constants of High Polymers," by C. O. Beckmann and J. L. Rosenberg, Columbia University.

NEW MEMBERS

Elected from May 16, 1944 to October 6, 1944.

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TRANSACTIONS

of

THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 7 DECEMBER, 1944 No. 2

SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 6, 1944

DOCTOR S. H. KNIGHT, Professor of Geology, University of Wyoming, Laramie, Wyoming: *The Physical Evolution of the Rocky Mountains of Southern Wyoming.* (This lecture was illustrated by black-board diagrams.)

The Rocky Mountain Region was pictured as it existed prior to the first orogenic disturbances in Late Cretaceous time. The rocks involved in this disturbance consisted of (1) a thick succession of fine textured Mesozoic sediments resting conformably upon (2) a thinner succession of Paleozoic rocks which in turn rested unconformably upon (3) the pre-Cambrian complex. The Mesozoic rocks were described as an alternating succession of marine and continental sediments which were derived from highlands lying to the west of the Rocky Mountain Region. Crustal warping was pictured as having produced elongate islands which rose in the Late Cretaceous sea which was thereby broken into a series of embayments and estuaries. The island uplifts were attacked by erosion and the resulting fine textured rock debris derived from the older sediments was deposited in the adjacent downwarped basins. The upper portion of the thick Mesozoic succession now exposed on the basin floors was pictured as having been reworked sediments locally derived from the rising mountain flanks Conglomerates derived from the lower part of the Mesozoic and underlying Paleozoic rocks which occur a few hundred feet above the last marine Cretaceous

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faunas were given as evidence that the mountains had suffered prolonged erosion before the final withdrawal of the Cretaceous sea. The time of the final withdrawal of the sea, with respect to the depth of erosion in the mountains, is not known.

Uplift and erosion of the mountains continued throughout Paleocene time and coarse-textured debris composed of mixtures derived from the pre-Cambrian and the flanking sediments were deposited along the flanks of the mountains while finer sediments were laid down in the central portion of the basins. The mountains gradually gained elevation with respect to the basin floor throughout Paleocene time. Intense orogenic movements at the close of Paleocene time resulted in extensive folding, thrust and tear faulting. The mountains were pictured as having reached their greatest relative height at this time. During Lower Eocene time extremely coarse-textured conglomerates which grade rapidly basinward into fine sediments were deposited upon older rocks. The marked unconformity between the Lower Eocene rocks and underlying rocks was shown to become less apparent when traced basinward. The last compressional movements which folded the rocks occurred following the deposition of the Lower Eocene succession. These rocks were locally folded although they remain in nearly a horizontal position over large areas.

Middle and Late Eocene rocks are absent, and the record of events is therefore less well documented. The following conditions were illustrated: (1) Erosion greatly reduced the mountains and broad erosional surfaces were cut on the pre-Cambrian rocks. (2) The basins were deeply excavated and the mountains were dissected. During Oligocene time, reworked volcanic ash partially filled the basins and the mountain canyons. These deposits covered the lower mountain drainage divides. Some coarse-textured debris from the mountains was intercalated with the fine ash deposits. The Oligocene ash beds were covered with conglomeratic and arkosic debris mixed with ash in Miocene time. The region was pictured as having suffered regional uplift in late Miocene time with the accompanying superposition of the drainage. Little is known of the Pliocene history. Some Pliocene sediments were deposited. Mention was made of the occurrence of local erosional surfaces cut at various elevations and at various times during the final excavation of the basins.

SECTION OF PSYCHOLOGY

NOVEMBER 20, 1944

DOCTOR WOLFGANG KOEHLER, Department of Psychology and Education, Swarthmore College, Swarthmore, Pa.: *New Facts in Visual Perception.*

In 1935, J. J. Gibson discovered several "figural after-effects." He found that when the middle of a slightly curved line had been fixated for some time, a straight line in the same location and orientation appeared curved in the opposite direction. From this and similar observations he concluded that, under conditions of prolonged inspection, lines which deviate from a norm, for instance, from the straight line, change in the direction of the norm; and that, as a result, the norm itself is correspondingly distorted if it is shown in the same part of the visual field.

More recently, Dr. Wallach and I have shown that, after prolonged inspection of any specific object, the metric characteristics of the visual field, both within and around the area of that object, are temporarily changed. The only condition which must be fulfilled is that the brightness of the object differs from that of the ground. It is not necessary that the object represent a deviation from a visual norm. Inspection of norms themselves gives rise to after-effects when proper test objects are chosen.

Within a given inspection object, various after-effects can be demonstrated, depending upon the choice of the test pattern and of its position relative to the area of the previously inspected object. This makes it advisable to distinguish between two facts: (1) *Satiation.* After inspection of an object, the parts of the visual cortex in which this object is physiologically represented are in a changed condition. It is here assumed that a visual figure is associated with a specific figure process, and that continuation of this process in a given area causes the changed condition of the medium. (2) *Figural after-effects.* The figure process of a test object reacts upon that changed condition so that the appearance of the test object is affected.

Apart from minor symptoms, figural after-effects consist in *displacements.* Test objects are either displaced as wholes, or parts of

these objects are shifted. In the latter case, test objects may suffer changes of size, or they may also be distorted. The direction of the effects is that test objects (or parts of such objects) recede from highly satiated regions of the field into less affected regions. Satiation is maximal within, and in the neighborhood of contours or outlines. If the inspection object is a circle and the test object a smaller concentric circle, this smaller circle will appear too small. Its contour moves inward, away from the maximally affected zone. Conversely, if the inspection object lies within the area of the test object, the latter will for the same reason appear too large. The changes are observed and measured by comparison with like objects in an unaffected region of the field.

When a test object is displaced, the degree of its displacement depends upon its distance from the previously inspected object. Within certain limits, a test object recedes less when it lies nearer the inspection object than it does at greater distances. In other words, the effect grows as the test object is placed in positions which are more and more on one side of the affected area. However, since, at very great distances, the process of the test object becomes independent of the affected area, displacements are maximal at a certain distance.

From this, it follows that if the parts of a straight test line lie at varying distances from an inspection object, such a test line must turn in space. It may turn as a whole, or its parts may turn in varying degrees so that a curve results. Both effects can be easily observed. In fact, Gibson's observations are examples of such effects.

Gibson's further finding that during prolonged inspection figures may change their *own* shape, can also be derived from the same principles: If satiation is greater on one side of an inspection line than it is on the other, the line will move in the latter direction. As a consequence, great caution is indicated, whenever for the purpose of accuracy in measurements visual patterns are shown a great many times in succession. Under these circumstances, the size, the shape, and other characteristics of the patterns are likely to undergo a gradual change by self-satiation. Thus, the disappearance of many visual "illusions" under conditions of repeated observation need not be a "practice" effect; for, in such patterns, satiation must be expected to operate against the asymmetries which constitute the illusions.

Quite recently, we have found that figural after-effects occur in the third dimension of visual space just as they do in the first two.

Practically any effect which has been observed within a plane parallel to the observer can also be demonstrated as a distortion at right angles to this plane. The only major difference is that, in the latter case, the phenomena tend to be more conspicuous. Where the order of magnitude of an effect, in the former case, is a few millimeters, the corresponding effect in the third dimension amounts to several centimeters.

It takes no more than a few seconds to establish a satiation which causes a figural after-effect, and the amount of the effect does not grow if the inspection period is prolonged beyond a few minutes (Gibson). By a summation technique, it can be shown that some satiation originates within a fraction of a second. On the other hand, satiation persists during periods which are enormous in comparison with the inspection time during which it was established.

Satiation can not be interpreted as a retinal change. Although some such effect may occur at any level, the main effect, undoubtedly, has a more central location. If, during the satiation period, only one eye sees the inspection pattern, a clear after-effect will be observed if the test pattern is shown only to the other eye (Gibson). At the same time, the effects are localized in the sense that distortions occur only within the affected area or in its neighborhood. The spatial coordinates which are here decisive are those of the retina or, rather, those of the visual cortex. This holds also for the effects in the third dimension.

Measurements show that individual differences as to the amount of the distortions are very great. This amount also varies from one part of the field to another. For instance, it seems to be greater in the periphery.

We do not believe that an explanation of figural after-effects can be given in terms of traditional neurophysiology. An interpretation in less conservative terms has been suggested elsewhere (4, 5, 6). Quite apart from any particular theory, the existence of figural after-effects seems to prove that specific objects in visual space are associated with specific processes (figure processes), which alone cause satiation. This fact appears to be incompatible with the notion that specific objects are established by learning, *i.e.*, that they have no genuine visual existence. Similarly, the occurrence of strictly analogous after-effects in the third dimension excludes all theories according to which this dimension has an indirect origin and is therefore not actually a visual datum. Again, the so-called "cues" for localization in the third dimension seem to

operate just as directly as does binocular parallax. For, so long as depth is clear, figural after-effects in the third dimension can be easily demonstrated when parallax is absent during the inspection period.

REFERENCES

1. **Gibson, J. J.**
   1933. Jour. Exper. Psychol. **16**: 1.
2. **Gibson, J. J. & M. Radner**
   1937. Jour. Exper. Psychol. **20**: 453.
3. **Gibson, J. J.**
   1937. Jour. Exper. Psychol. **20**: 553.
4. **Köhler, W.**
   1938. The Place of Value in a World of Facts. Liveright Publ. Corp.
5. **Köhler, W.**
   1940. Dynamics in Psychology. Liveright Publ. Corp.
6. **Köhler, W. & H. Wallach**
   1944. Proc. Am. Philos. Soc. **88**: 269.

DOCTOR DONALD MARQUIS, Director of the Office of Psychological Personnel, National Research Council, Washington, D. C.: *Psychology in the War.**

The growth of psychology since the last war has provided a much wider opportunity for its contribution in the present war. The major task of professional psychologists in the Armed Services is research on the human factor in connection with the following problems: (1) the analysis of military tasks and occupations; (2) the development of tests and procedures for selection and classification of personnel; (3) the development of training programs and methods of evaluating training; (4) the design of instruments and weapons from the standpoint of the traits and capacities of the personnel using them; (5) the development of clinical techniques and procedures for individual examination and consultation services; (6) the study of psychophysiological factors, such as vision, hearing, fatigue in the performance of specialized military tasks; (7) the development of techniques and procedures in the orientation program, in morale services, and in psychological warfare. Military psychologists also undertake the supervision of the administration of practical programs arising from the results of their research investigations. Routine duties of group-test administration and scoring, interviewing, classification, and assignment, are carried out by commissioned and non-commissioned personnel receiving the necessary special training in service schools in which psychologists participate as instructors.

When preparation for the present war was undertaken in 1940, the Services were without qualified psychologists or established research programs in this field. Beginning with a few individuals in key positions, the Army, Navy and Air Forces now utilize more than one thousand qualified psychologists in specialized work and some 250 more are serving in civilian status in the War and Navy Departments and in the work of the Office of Scientific Research and Development. In addition, the Army has trained 1300 enlisted men in advanced personnel psychology in the Army Specialized Training Program in the universities.

The nature of modern warfare makes it essential that research on military service problems be maintained at a high level of efficiency

* This address was delivered at the meeting of the Section of Psychology, October 16, 1944.

following the present war. Serious consideration must be given to the promotion and organization of military psychology in the postwar period.

SECTION OF ANTHROPOLOGY

NOVEMBER 27, 1944

DOCTOR STANLEY NEWMAN, Language Section, Information and Education Division, War Department, New York, N. Y.: *Cultural and Psychological Features in English Intonation.*

The purpose of this paper is not to present a picture of English intonation for its own sake, but rather to use the intonational phenomena of English as an illustration which can provide us with some insights into the functioning of language. The intonations afford a very convenient kind of illustration. They can be neatly, though arbitrarily, fenced off from other kinds of language phenomena. By focusing our attention on them, we can see in miniature and in tangible detail how language can function on several different levels.

When a person is talking English, a great many things are going on at the same time. On one level, he makes use of the linguistic system of English. The linguistic system, as the linguist describes it, is a purely formal system. It is made up of the phonetic pattern of the language, the changes that the sounds of the language undergo in word formation, the sequences in which these sounds can be placed. The sounds he uses are elements in a configuration of sounds that is peculiar to English and to no other language. In the linguistic system are also included the meaningful elements of the language, and the processes which govern the combining of these elements into words and sentences. The configuration of meaningful elements, too, is part of the linguistic system. When a person talks English, he draws upon the configurated linguistic system of English, which is distinctive for the English language, differentiating it from all other languages.

As he is talking English, however, he is doing more than just actualizing the linguistic system. In his speech, he is also actualizing patterns of behavior which he expresses in other, non-speech areas of activity. To take an obvious example, a hyper-active person, whose bodily movements are energetic, will usually have an energetic and vigorous manner of articulating sounds. A slow-moving person will usually have a lax articulation in speaking. The vigorous or lax character of articulation, in other words, is not a part of the linguistic system of English, even though we have vigorous and lax speech in English. This feature of articulation, however, is a part of the indi-

vidual personality system. It is related to other features of that individual's behavior.

In the same way, cultural features also find expression in language. Here we must be a bit more cautious. In our own culture, it is an ancient and highly respected practice to make cultural statements about language as if they were strictly linguistic statements. This practice is carried on by our schools, which are necessarily concerned with teaching culturally approved attitudes toward the use of language and with developing culturally useful skills in language, such as reading, spelling, and writing All of us are exposed to this tradition, especially from the time we enter school. And, unless we are on our guard, we can be easily led into the fallacy of assigning culturally defined features to the linguistic system.

To take an example of this type of fallacy, the statement is made that using the word *ain't* is not good English. In our schools, we are not only taught notions of this sort, but we are made to believe that these notions form an essential part of English grammar. To the linguist, of course, forms are neither good nor bad; they merely exist or do not exist. His linguistic description would attempt to place the word *ain't* in its proper configuration of forms. He would say that among the irregular verbs of English there is one which has three forms in the present (or non-past) tense: *am, is, are.* The negative is formed by inserting *not* after the verb form or by a contraction of the verb with the negative. He might find that in three different dialects— dialects A, B, and C—the contracted negatives of this verb were patterned in the following way:

	A	B	C
am not	ain't	ain't	......
is not	isn't	ain't	isn't
are not	aren't	ain't	aren't

To the linguist, there is nothing about the word *ain't* that would lead him to reject it as an acceptable part of the English linguistic system. All of the sounds in *ain't* are perfectly good English sounds. There is nothing in the verb paradigm that conflicts with its position in that paradigm. As a matter of fact, its absence (as in dialect C) leaves a vacuum in the pattern of forms.

To state that *ain't* exists and that it is a perfectly acceptable form, on linguistic grounds, in the structural system of English does not dispose of the *ain't* problem. But it does clarify the problem. It

presents the linguistic side of the case. The statement, on the other hand, that *ain't* is not good English fails to make linguistic sense because it is not a linguistic statement at all. It is a cultural statement. It has reference to the fact that certain groups in our society consider *ain't* to be a symbol of vulgarity. Once the statement is recognized as having a cultural relevance, not a linguistic one, it makes sense and takes on meaning. In terms of culturally defined attitudes, *ain't* belongs with gum-chewing and with other practices that are supposed to indicate a lack of refinement, not with the linguistic forms *isn't* and *aren't*.

Cultural evaluations of this kind have a reality and a legitimate place in a cultural frame of reference. Such evaluations cannot be argued out of existence by pointing to the linguistic validity of *ain't* or to the physiological validity of gum-chewing, which may aid the digestive process and ease nervous tension. Cultural validities very often do conflict with other kinds of validities. In specific social situations, we are constantly being called upon to resolve such conflicts. The growing child goes through a long and often painful process of learning how to adapt his physiological demands to conflicting cultural demands.

The cultural definition of *ain't* and gum-chewing fits quite naturally in a stratified society, where certain groups consider themselves to be superior to other groups, where practices in talking, eating, modes of dress and grooming, housing, etc. become charged as symbols of status. Such practices may indicate, not only the social stratum to which we belong, but also our affiliation in terms of sex, age-group, and other types of culturally defined classifications.

The evaluation of linguistic forms, such as *ain't*, on the basis of cultural classifications, is only one item in a highly elaborate network of culturally defined notions about language. This culture complex with regard to language is not limited, of course, to speakers of English. It is found among speakers of Chinese, of Arabic and Hindustani, as well as among speakers of the European languages. It is spread over a culture area that includes the so-called civilized or non-primitive cultures of today, and it has a history that must be measured in millenia, not merely in centuries. It includes many associated ideas about language: that the way in which the spoken language is used, not only indicates the social stratum of the speaker, but also his education and learning; that a native speaker does not really know his language unless he has learned the folklore of grammar and rhetoric about his language, with

all its prohibitions and exhortations; that the real language is the written form, the spoken form being merely a pale reflection of the writing; that the essence of a language is to be found in its literature, particularly in its belles lettres; and that, therefore, a society which has no written literature has no real language.

To turn now to the intonations of English, I want to give first a description of the linguistic patterns of pitch phenomena in English and then pass on to the psychological and cultural features of these phenomena. English has four distinctive intonation patterns that can be described in linguistic terms.

1. *Rising intonation.* A rise in pitch occurs on the last primary-stressed syllable of questions which can be answered by yes or no. For example:

> Is his name John?
> Do you live in Mahattan?
> Is this the way to the railroad station?

Note that the nucleus of the rising tone is the last primary-stressed syllable—*John*, *-hat-*, and *rail-* in the sentences above.

2. *Falling intonation.* A fall in pitch occurs on the last primary-stressed syllable of statements and of questions that contain interrogative words, such as *where, when, how, why,* etc.

> I live in Manhattan.
> Where is the railroad station?

3. *Falling-rising intonation.* A falling tone occurs on the last primary-stressed syllable followed by a rising tone on the final syllable of the sentence. This intonation has the function of expressing surprise or doubt, apology, and the like.

> You live in Manhattan!
> I won't be able to drive you to Boston, but I can take you to the railroad station.

If these sentences are spoken with the intonation that is indicated, they will differ in pitch from the preceding two sentences only in that the final syllables of *Manhattan* and *station* will have a rising tone.

4. *Final-rising intonation.* A rising tone occurs on the last syllable of the word or word group, regardless of stress. This intonation is

found, among other places, in certain English tag constructions that can appear at the end of sentences.

I'll be able to do that, I think.

"Where do you live?" he demanded.

Close the door, Albert.

In these sentences the rise in pitch occurs on the final syllable *think,* which happens to be primary-stressed, and on the final syllables of *demanded* and *Albert,* which do not have the primary stress.

There are two general characteristics that should be noted about this linguistic description of intonation. First, these intonations are not only described in terms of pitch contours, they must also be described in terms of the location of these pitch contours with reference to the primary stress and to syllabic position. In other words, these intonations are correlated with the stress pattern of English and with the set-up of syllabic positions. The stress pattern, in its turn, is correlated in certain describable ways with the vowel configuration of English, with the consonant sequences of English, with the behavior of certain English prefixes and suffixes, with certain syntactic relations in English. The linguistic system that we call English is a close-knit structure made up of inter-related formal features. The intonations described above are linguistic features by virtue of the fact that they are related to other linguistically relevant features and are woven into the total linguistic system.

The second general point is that these four intonations do not describe all of the phenomena of pitch which occur when a person talks English. They merely represent the formal, linguistically definable constants of intonation. For example, the description of the rising intonation does not state how much of a pitch interval this rise covers. Some speakers of English habitually use a wide span of pitch; others use a relatively narrow span; still others vary considerably from day to day or even from one response to another. The point is that such differences in pitch span do not count for anything in the linguistic picture. They are linguistically non-significant variations on the same theme. Whether your rise at the end of the question is wide or narrow, it still functions linguistically as the same type of question intonation.

These two general criteria can also be applied to the psychological features of pitch phenomena. It is necessary to select those features of intonation which correlate with other psychologically relevant phenomena. It is likewise important to omit those intonational features

which are psychologically non-significant. I shall try to indicate how the psychological features of intonation shape up by giving a summary of an investigation conducted in collaboration with Dr. Vera Mather, of the Psychiatric Service of the New Haven Hospital. The purpose of this study was to determine what speech characteristics were associated with certain types of clinically defined symptoms. In order to limit the problem, the study was confined to patients with disorders of affect—patients who showed clear symptoms of motor retardation or acceleration, and depression, euphoria, or irritability. About 40 patients with manic-depressive and allied syndromes were selected for this study. Phonograph recordings were made of the patients' spontaneous speech, in conversation with the psychiatrist. In a few cases, where it was possible, recordings of the same patient were made at intervals to see whether changes in his clinical manifestations were reflected in corresponding changes in his speech.

A list of speech characteristics was drawn up to serve as the criteria for description. In addition to pitch phenomena, this list also included some of the phenomena of articulation, accent, tempo, resonance, vocabulary and phrasing, syntax, etc. Only the features of pitch, however, will be dealt with here. The pitch characteristics which turned out to be significant for this psychiatric study were the following:

1. Range of pitch: wide, narrow,
2. Pitch changes
   a. Character: gliding, step-wise
   b. Frequency: frequent, infrequent.

The patients were classified into three clinically defined groups. One was the group showing symptoms of the classical depression, such as motor retardation, sadness of mood, insomnia; and, in some cases, agitation, and self-accusatory or suicidal trends. Some of these patients had experienced similar illnesses before. A few had had previous manic phases.

The second group was made up of patients showing a chronic state of dissatisfaction, self-pity, and gloom. This state, unlike the classical depression, was not a circumscribed illness. It did not follow a definite course. Patients in this state showed some changes of mood in response to changes in the situation, while patients of the classical depression group showed no such adaptive responsiveness in mood. Furthermore, there were no clear-cut somatic symptoms associated with this condi-

tion, in contrast to the insomnia, anorexia, and constipation which were usual in patients with classical depressions.

The third group, the patients with manic symptoms, was characterized predominantly by motor acceleration and euphoria or irritability. Many of these patients had had similar attacks before or had experienced depressive phases.

The features of pitch were found to be distributed among these three clinical types as follows:

	Classical depressions	*States of dissatisfaction*	*Manic syndromes*
Pitch range	narrow	wide	wide
Pitch changes			
Character	step-wise	gliding	gliding
Frequency	infrequent	frequent	frequent

On the basis of pitch characteristics, patients suffering from states of dissatisfaction were more like the manic than like the depressed patients. It should be remembered, however, that only the characteristics of pitch are being described here. By themselves, they give a skewed version of the total speech picture. In terms of other speech characteristics, patients in states of dissatisfaction showed more resemblance to the depressed patients. For example, patients in this state, as well as depressed patients, used a level of style that was a simple colloquial type of spoken English; manic patients, on the other hand, used a more involved and elevated style, often verging on the oratorical.

For some of the patients in the depressive and manic groups, two or three recordings were made at intervals of several weeks. Clear-cut changes in the clinical picture can generally be observed in patients of these types. Over a period of weeks or a few months, many of these patients undergo a hospital recovery, as their symptoms of motor retardation or acceleration, of sadness or euphoria, of agitation or irritability become less marked. Changes are also manifested in speech. To take an example, three recordings were made of a depressed patient during his recovery period of about ten weeks. His successive recordings showed a widening range of pitch and a more frequent use of pitch changes; in the last recording, taken on the day of his discharge from the hospital, a few gliding tones appeared, whereas this patient had previously used only step-wise changes of pitch. A manic patient showed corresponding changes in the opposite direction during a rapid recovery period of about six weeks—a narrowing of the pitch range, less

frequent changes of pitch, and the use of short gliding tones in place of the previous long-drawled glides.

The characteristics of pitch selected in this psychiatric study must not be over-interpreted. They should not be regarded as being representative of the totality of speech, or as the most important characteristics or the best diagnostic clues in speech. They are an arbitrarily selected set of speech characteristics. I want to avoid misleading an unwary reader who might be tempted to believe that it is possible to psychologize a person merely by describing his intonations. Furthermore, in the group investigation just described, the characteristics of pitch could be shown to correlate very clearly with certain clinical syndromes. The uniformity of speech characteristics is the result of the type of subjects used and of the group nature of the investigation. A more complex and, in many ways, a more rewarding picture emerges from a detailed individual study, in which non-psychotic persons are used as subjects. Here, the speech material may fall into several functional sets of characteristics, which are related, not on the basis of uniformity, but rather on the basis of congruence or even opposition. This type of speech picture can reflect the compensatory relations, the conflicts and ambivalences, in the character structure of the individual.*

The culturally significant features of intonation shape up in quite a different way. When I began working on English intonation, as part of a larger linguistic study of English, I proceeded in the accepted inductive manner, recording and collecting the various intonational patterns that I heard. Several curious patterns turned up. For example, one of the patterns I heard was that used by children who would make it into a chant for teasing or taunting other children. It would occur in such sentences as "Johnny is a slow-poke, Johnny is a slow-poke." Another peculiar pattern was the one used in military commands. When commands such as "Attention!" or "Forward, march!" are given with this intonation, the final syllable is spoken with a sharp high tone, and the preceding syllable is drawled on a low tone. In the discussion of the linguistic features of intonation it was pointed out that these features were an integral part of the linguistic system, correlated to the primary stress and to the position of syllables. The peculiar nature of the taunting pattern used by children and the pattern of military com-

* The report of the psychiatric study will be found in **Stanley Newman & Vera G. Mather**, *Analysis of spoken language of patients with affective disorders.* American Journal of Psychiatry **94**: 913-942. January, 1938. The individual study referred to is described in my paper, *Behavior patterns in linguistic structure: a case study.* Language, Culture and Personality: 94-106. Menasha, Wisconsin: Sapir Memorial Publication Fund. 1941.

mands lay in the fact that neither of these patterns seemed to be related to the genuinely linguistic patterns of intonation or to any other formal features of English. In the taunting pattern, the fixity of the pitch intervals and the constancy of the melodic outline throughout the sentence was more suggestive of a musical melody than of a flexible linguistic intonation. In the pattern of military commands, as a matter of fact, the normal distribution of stress was distorted. The last syllable was always spoken with the strongest force of articulation, even in words such as *attention,* where the primary stress is normally on the middle syllable.

It soon became evident that neither of these patterns was part of the English linguistic structure. The taunting melody is obviously the same as the melodic theme of the phrase, "a green and yellow basket," in the nursery song, "A Tisket, A Tasket." Folk tunes of this kind are scarcely to be regarded as an integral part of the linguistic system of English. They are probably very old elements belonging to a European musical tradition that is shared by cultures in which different languages are spoken. The same melody is used with the same function of taunting by children who speak German, and it will undoubtedly be found among children of many different languages.

In the same sense, the military pattern is a cultural item. It is used, not only in German, which is closely related to English linguistically, but also in Persian, which is only remotely related, and in Turkish, which is not related to English at all. This technique of intoning military commands is obviously a part of a military culture complex which has been spread widely over the world. It crosses linguistic boundaries and cannot be considered a part of the English linguistic system.

Two main types of culturally relevant intonation can be distinguished. On the one hand, there are the complex intonational styles, involving not only pitch but also characteristics of articulation, tempo, resonance, etc. Here are included the styles of speech associated with certain personality stereotypes current in our culture—the voice of the preacher, the doctor, the lawyer, the politician, the silly woman, the braggart, the dunce. The stereotype of the dunce or stupid fellow appears in folk-tales all over Europe. I have heard Czech stories in which the speech of this folk individual is characterized by the same low, long-drawled glides, the same laryngeal resonance, the same hesitation pauses in Czech that I now hear in English as spoken by "Mr. Finnegan" of the

Duffy's Tavern radio program. He represents an old traditional folk character with an old traditional way of speaking, regardless of what language he happens to be using. In addition, there are certain ways of using intonation and other voice characteristics to indicate culturally standardized emotional attitudes—to indicate politeness, impatience, sophistication, anger. There are certain styles employed for certain culturally defined situations—the style used in prayer and in pulpit utterance, the style which some adults consider appropriate when addressing small children, and one which some children learn to imitate.

In contrast to these styles which involve certain types of methods of using intonation are the more distinctive melodies. These include the taunting melody used by children and the melody of military commands, mentioned above. There are also calling melodies, used particularly in calling out proper names. One can hear these every day intoned by mothers on our city streets. And there are special melodies used by street vendors—the old clothes man, the scissors man, etc.

I should like to sum up this treatment of intonation by suggesting that the general picture of intonations given here may apply not only to the pitch phenomena of English but to other language phenomena as well. First, there are the linguistic features that occur in language, features that are related one to another and go to form the closely knit structure that we call the linguistic system. There are, in addition, the psychological features appearing in an individual's use of language. These are the phenomena of language that give free play to the individual's modes of behavior, which are also expressed in and correlated with types of activity other than speech. They reflect his motor habits and also his mental (emotional and intellectual) habits. Finally, there are the cultural features found in language. They are not a part of the structure of any particular linguistic system. In their geographical distribution, they cut across linguistic boundaries and coincide, rather, with culture areas and with the spread of culture complexes. They undoubtedly, too, have a history of their own, independent of the history of the particular language in which they happen to be used.

SECTION OF BIOLOGY

November 10 and 11, 1944

Conference on *"Animal Colony Maintenance."*

The Section of Biology held a Conference on "Animal Colony Maintenance," as the second in the series for the Academic year, 1944–1945. Doctor Edmond J. Farris, The Wistar Institute of Anatomy and Biology, Philadelphia, Pennsylvania, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by Edmond J. Farris.

"Genetic Purity in Animal Colonies," Frederick B. Hutt, New York State College of Agriculture, Cornell University.

"The Mating of Mammals," by Carl G. Hartman, University of Illinois.

"Feeding Laboratory Animals," by J. K. Loosli, School of Nutrition, Cornell University.

"Infectious Diseases of Laboratory Animals," by Herbert L. Ratcliffe, Medical School, University of Pennsylvania and Penrose Research Laboratory, Zoölogical Society of Philadelphia.

"Remarks," by Edmond J. Farris.

"Influence of Environmental Temperatures on Warm-Blooded Animals," by Clarence A. Mills, College of Medicine, University of Cincinnati.

"Financing and Budgeting—Viewpoint of the University," by Sidney Farber, Harvard Medical School.

"Financing and Budgeting — Viewpoint of the Commercial Breeder," by C. N. W. Cumming and F. G. Carnochan, Carworth Farms, Inc., New City, N. Y.

NEW MEMBERS

Elected November 16, 1944

ACTIVE MEMBERS

Kattenhorn, Dorothy Helen, B.A., Geological Assistant, Columbia University, New York, N. Y.

King, James C., Ph.D., Entomology and Population Genetics, Private Research in Entomology (Sphingidae) and Population Genetics, White Plains, New York.

Kirkpatrick, George P., B.Sc., Astronomy and Physics, Associate Physicist, Brooklyn Polytechnic Institute, Brooklyn, New York.

Mazur, Abraham, Ph.D., Biological Chemistry, Instructor in Chemistry, College of the City of New York, New York, N. Y. (on leave); Research Biochemist, Medical Research Laboratories, Edgewood Arsenal, Maryland.

Welsh, Mark, D.V.M., Infectious Diseases of Animals, Director, Veterinary Division, Lederle Laboratories, Pearl River, New York.

ASSOCIATE MEMBERS

DeFrates, Joseph S., Ph.D., Endocrinology, Department Head, Glandular Products, E. R. Squibb and Sons, New Brunswick, New Jersey.

Long, Charles R., M.S., General Science, formerly, Research Engineer, Westinghouse Lamp Division; now, Captain, U. S. Army.

McCleary, Harold R., Ph.D., Physical Chemistry, Research Chemist, Calco Division, American Cyanamid Company, Bound Brook, New Jersey.

Robinson, Leslie G., D.D.S., Osteology, Dental Practitioner, New Westminster, British Columbia, Canada.

TRANSACTIONS
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SER. II, VOL. 7 JANUARY, 1945 No. 3

JOINT MEETING

SECTION OF GEOLOGY AND MINERALOGY

AND

SECTION OF BIOLOGY

DECEMBER 4, 1944

DR. CARL O. DUNBAR, Professor of Paleontology and Director of Peabody Museum, Yale University: *The Geologic and Biologic Significance of the Evolution of the Fusulinidae.* (This lecture was illustrated with lantern slides.)

The Fusulinidae comprise a well defined family of the Foraminifera found in great abundance in the later Paleozoic rocks. They appeared near the beginning of Pennsylvanian time and lived through until late in the Permian, commonly existing in such numbers in the shallow seas of the Northern Hemisphere as to be important rock makers. They are thus among the commonest and most characteristic marine fossils of a number of the great coal and oil fields of the world. Because of their small size and vast numbers, they are easily recovered from well cores or even from drill cuttings, and so are especially useful in subsurface correlations.

Their usefulness in this way stems largely from the fact that their shells are complicated and, throughout the geologic range of the family, were undergoing very rapid evolution. This involved, among other things, a tendency toward greater and greater size and the progressive

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change in several shell features. A knowledge of these evolutionary trends, once it is integrated to the stratigraphic record, permits one to recognize the geologic horizon and to make correlations over great distances, even without the identification of species.

The fusulines are relative giants among the Foraminifera, commonly resembling grains of wheat or oats in size and shape, but ranging upward to subcylindrical shells as thick as a lead pencil and two inches in length. Compared with *Amoeba* or *Globigerina*, one of these largest forms would appear like a *Brontosaurus* of the protozoan world!

The fusuline shell is planispirally coiled about an elongate axis and is typically fusiform. Indeed, the original genus, *Fusulina*, was so named because of its shape, and most of the other genera agree with it in this respect, though with variations ranging to subcylindrical on the one hand and to subspherical or even lenticular on the other.

The spiral wall of these shells developed an amazingly complicated structure. Moreover, the whorls are subdivided by numerous radially disposed septa into slender meridional chambers. These septal plates, at first plane and simple, display various types of specialization. Secondary shelly deposits tended to form in characteristic but changing patterns; and the meridional chambers, primitively slender and simple, became subdivided into many chamberlets. All this complicated internal architecture is invisible from the exterior, and shells that appear superficially alike may be utterly dissimilar internally. For this reason, critical study of a fusuline requires at least two carefully oriented thin sections of the shell, one following the axis and the other the sagittal plane, both of them passing through the initial chamber. Cutting of such sections is a tedious task calling for skill and experience, and is one of the chief obstacles in the study of the fusulines.

In the evolution from primitively small and simple to relatively large and structurally complicated shells, several different structures were involved and each shows definite evolutionary trends more or less independent of the others. Some of these indicate a slow orthogenetic change persisting throughout a considerable amount of geologic time—a change of the sort displayed in the evolution of the horse from his pentadactyl and plantigrade ancestor to the unidactyl and unguligrade animal of the modern world. Such changes were transpiring simultaneously in various parts of the world and, in the case of the horse, clearly resulted in more perfect adaptation of the animal to its environment. One is tempted to infer that the major evolutionary changes

displayed by the fusulines also were, in general, adaptive; even though the environmental factors that gave them survival value are, in some respects, obscure.

One of the major changes had to do with the mural pores. In primitive genera—all those characteristic of the lower half of the Pennsylvanian system—these are microscopic capillary tubes having a diameter of only 1 to 3 microns and, therefore, being difficult to detect in ordinary thin sections. But near the middle of the Pennsylvanian period (i. e., late in the Des Moines epoch), the mural pores began to enlarge and, in shells of that age, they are commonly visible though still tubular. A rapid change followed, in which the main layer of the wall thickened and the mural pores enlarged into alveoli, like the cells in honey-comb, remaining fine only in a thin outer film of the wall (the tectum). The net result was to produce a relatively thick wall having the maximum rigidity attainable with a given amount of shell material, or the least possible weight capable of yielding a given amount of rigidity. The frequent discovery of specimens that were broken and healed during life suggests that the strength of the shell was frequently taxed; and, since all the genera that include large species have the alveolar type of shell wall, we may infer that lightness of contruction may have been a factor in limiting the growth of such unicellular animals.

The gradual development of septal folds is one of the striking evolutionary trends in the subfamilies Fusulininae and Schwagerininae, where it results in great strength with a minimum of weight, on the same principle that makes a corrugated paper carton light and strong. Curiously, the Oriental subfamily Neoschwagerininae achieved the same end in a quite different way. Successive stages in septal evolution can be used to distinguish several stratigraphic horizons.

Secondary shell deposits occur in many of the fusulines, but their characteristic distribution within the shell displayed systematic changes in the course of time. A knowledge of these changes also can be effectively used as a check on the geologic age.

At the beginning of the Permian period, two new genera of fusulines appeared, each of which is characterized by a very rapid increase in the height of its chambers at a certain stage in its ontogeny. There are *Pseudoschwagerina,* derived out of *Triticites,* and *Paraschwagerina* derived out of *Schwagerina.* In both, the early whorls are closely coiled as in other genera of fusulines; but, after this ontoge-

netic stage, the next whorl rapidly increases in height and the shell becomes inflated and commonly subspherical. Both these genera were enormously successful for a time, spreading rapidly throughout the seas of the Northern Hemisphere. Each had a rather short career, dying out without issue at the end of the Wolfcamp epoch. One is tempted to think that the sudden inflation of the shell marks an adaptation to pelagic life, the expanded chambers having been partly filled with gas as is the final chamber in the living foraminifer *Tretomphalus bulloides*.[1] This possibility may be indicated by the fact that the spiral wall of these inflated genera is much thinner than that of the parent genera, that these inflated shells are never weighted down with secondary shelly material, as are many species of the contemporaneous genus *Schwagerina*, and that they are so widely distributed.

[1] **Myers, Earl H.** Biology, Ecology, and Morphology of a Pelagic Foraminifer. Stanford University Publications, Biol. Ser. 9 (1): 1–30. 1943.

SECTION OF PSYCHOLOGY

December 18, 1944

Doctor Jules H. Masserman, Division of Psychiatry and the Otho S. A. Sprague Institute, University of Chicago, Chicago, Ill.: *The Biodynamics of Experimental Neuroses and Alcoholism.* (This lecture was illustrated by motion pictures.)

I don't believe it is necessary for me to express in words the deep appreciation I feel for this opportunity to address the Academy. Certainly, my presence here, this evening, despite every obstacle of crowded duties and difficulties of travel, should convey the fact that I consider this a signal—though undeserved—honor. And I shall try to show my appreciation concretely by fulfilling, as nearly as I can, the purpose for which, I presume, I was invited: namely, to present a concise over-all survey of our work on experimental neuroses at The University of Chicago, including the recent studies on alcohol which supply the title to this paper. With your permission, then, I shall omit technical descriptions of our methods or detailed analyses of our results, in the form of tables, graphs or mathematical derivatives, which, though entrancingly exact in form, are often dubious in meaning. Instead, I shall try, later on, to substantiate my statements by employing what is in many respects the most objective as well as the least tedious way of presenting vital data: i.e., by showing a motion picture film of some of the actual experiments under discussion.

The Hypothalamus

Let me, then, sketch as briefly as possible the developmental background of our studies. About a decade ago, my student associates and I began to correlate our clinical psychiatric work with parallel experimentation in animal behavior, in the hope of discovering biodynamic principles broad enough to cover the conduct of both animals and men. In line with these interests, it was our purpose to study those aspects of behavior that were of greatest significance from a psychiatric standpoint, namely: its biologic, motivational and emotional determinants. Our first experiments, accordingly, were concerned with a so-called "psychosomatic" theory which at that time seemed to be gaining headway among some physiologists and psychiatrists. In substance, this

theory held that motivations and emotions had their origin in the neural and hormonal functions of the hypothalamus—a tiny neural structure that thereby embodied the solution (as per *circa* 1935) of the body-mind problem and, in effect, represented the end of the ancient quest for the "seat of the soul." Purported evidence for this concept of hypothalamic function was derived mainly from what proved to be certain unwarranted interpretations of the brilliant work of Karplus and Kreidl, Cannon and Britton, Bard, Ranson, Rioch and others. These investigators, in brief, had shown that stimulation of the hypothalamus in animals produced the peripheral expressions of what Bard rightly called the pseudo-affective state of "sham rage," whereas lesions of the hypothalamus rendered the animal for a time emotionally apathetic. I shall not go into an extensive exposition of our own experiments in this field, since they have already been filmed and reported in, perhaps, too great detail elsewhere,* but certain of our results may be briefly reviewed as relevant to the present discussion. In short, we found that the dramatic "sham rage" responses produced by electrical or pharmacologic stimulation of the hypothalamus, unlike those in true affective states, were mechanical, unadaptive and did not interfere with ordinary goal-directed behavior such as conditioned food-taking; whereas, unlike true rage or fear, these pseudo-affective responses could not be conditioned to situational stimuli. Conversely, animals rendered apathetic by destruction of the hypothalamus regained their normal emotional responses as soon as they recovered from the metabolic disturbances produced by the initial neurologic lesions. These and other experiments indicated that motivation and emotion are not to be localized in particular body structures, nerve pathways or blood hormones, but are expressions of holistic adaptations to the total field of the organism.

Our further work, therefore, entailed the development of techniques that would make possible the study of the motivations, field organization and general adaptive nature of total behavior—terms that I hope to make clear as this discussion develops. For this purpose, as you will see in the films, we designed and built an apparatus that made possible a wide variety of controlled experimental procedures, such as: (1) the use of known incentives to action; (2) the employment of field

* For an extensive bibliography and discussion, see **Masserman, J.**, "Behavior and Neurosis," Chicago, University of Chicago Press, 1943. The experiments themselves are recorded in several teaching motion picture films from the Division of Psychiatry of the University of Chicago, distributed by the Psychological Cinema Register, State College, Pa.

symbolizations to "condition" specific adaptive patterns; (3) the interposition of physical obstacles or "social" frustrations; and, finally, (4) the elicitation of simultaneous but incompatible responses in order to study their disruptive effects on the animal's behavior. Let me now describe some of our basic observations with these procedures as employed with some two hundred and fifty animals during the past six years.

Experiments in the Biodynamics of Behavior

Motivation and Symbolic Adaptation

In our early experiments we found that a cat or a dog could easily be taught to lift the lid of a food box to secure a pellet of food, and then further trained to delay this response until a bell or a light signal had been given. During this period, the animal readily entered the apparatus, responded avidly to the signals *until it wanted no more pellets* and, throughout, remained on the best of terms with the experimenter.

Development of Complex Action Sequences in Adaptive Behavior

The animal could next be trained to depress a pedestal switch to actuate the feeding signals and so secure food at will. Finally, this adaptive behavior could be made progressively more complex by interposing barriers or by making the switch manipulation more difficult up to the limits of the perceptive and adaptive capacities of the animal. For instance, one cat was taught to pass a barrier twice and depress the switch three times in various positions before attempting to feed,—a level of performance which placed the animal in the "genius" bracket of the general cat population. Significantly, if the circuit was disconnected so that the feeding signals were not actuated when the switch was depressed, the total adaptations of the animal were in most cases disrupted, so that it did not attempt to secure food freely available in the box until the proper feeding signals were again interposed. However, as long as the environment remained organized and experimentally predictable, the animal went contentedly about its task of working the switch, responding to the signals and earning its living in its accustomed ways.

Effects of Frustration

If the food box were locked or barriers were so placed that an

animal was deprived of its reward after working the switch, the animal at first attacked the obstacles in its path with an intensity roughly proportional to its degree of hunger motivation. When these attacks did not succeed, it showed interesting explorations in *substitutive activity:* for instance, it would press down on ropes or other objects in its cage instead of the signal switch, or it would reach for the lights and bell in lieu of the unobtainable food. In this connection, a particularly significant form of substitutive aggressivity between one animal and another appeared in circumstances of "social thwarting" which could be arranged experimentally as follows:

Dominance and Aggression

Two trained cats were placed in the cage together and given only a single pellet after each feeding signal. Under such conditions, the more alert and active of the two pre-empted the pellet, whereas the other, with little or no fighting, soon ceased to respond to the signals and thereafter surrendered the food rewards to its dominant partner. In this way, an "hierarchy of privilege" could be set up in a group of two, three or four cats, so that the second in line attempted to feed only when the dominant one was removed; the third, only when the top two were gone; and the last, only when it was alone in the cage. When, however, an animal accustomed to dominance was superseded by an even more dominant animal from another group, the displaced animal ceased to respond to the signals, but appeared to find substitutive outlets in the form of violently aggressive attacks on the interloper. Significantly, also, the newly dominant animal in the group rarely responded with counter-aggressions as long as it had free access to the food-rewards, but when it, also, was socially thwarted in this goal-directed activity, it, too, became deviantly aggressive.

Other and even more interesting patterns of inter-cat relationships developed when two animals trained in the use of the signal switch were placed together, with the apparatus so arranged that the animal which worked the switch could not secure the single pellet before its partner did so. In such circumstances, some animals for a time worked the switch alternately so as to feed each other, but sooner or later this arrangement broke down and one of the animals began to shirk its share of the work to become "parasitic" on the other. In most pairs, however, the "worker," too, later quit for lack of reward, and then both animals lay about starving rather than attempt further cooperation.

Often, however, the worker, by turning his aggressions on the switch, learned that if he attacked it violently enough so that many pellets fell into the box, he could get some before the parasitic animal had consumed them all. From then on, he generally continued to work the switch many times, got a part of the proceeds for his efforts, and so once again stabilized the situation. Observations even at this level of animal relationships, therefore, seemed to support the sociological dictum that individuals in groups tend to fall into hierarchies of dominance-submission and worker-parasitism, and that aggressions appear in an individual subjected to social displacement and frustration. Our studies in these respects, then, confirmed and extended the observations of Mowrer,* and supported the conclusions he presented before this Academy some four years ago.

Experimental Neuroses

One other aspect of our recent work also has an immediate bearing on our discussion tonight, namely: the production of experimental neuroses in animals by the method of inducing conflictful motivations leading to disparate and incompatible adaptive patterns. A typical experiment may be described as follows:

An animal is trained to depress a signal switch before opening a box for a pellet of food. Just as the animal is about to secure its reward, however, a blast of air is unexpectedly blown across the box, or a mild condenser shock is administered through the grill floor of the cage. The animal recoils at this disturbing experience, but, after some hesitation, again essays to feed at the signal. This is permitted a few times but, later, and again unexpectedly, the air blast or shock is repeated. Eventually, then, the hungry animal must face a motivationally insoluble problem. Each signal indicates that food is available if the animal were to open the box as usual, but now the same signal also represents (or "means") an intolerable self-frustration in either feeding or escape. In this dilemma, the animal develops aberrations of conduct that correspond so closely to the phenomena of human neuroses that I have dared the wrath of my psychoanalytic friends by describing them in clinical terms. For instance, the animal develops feeding inhibitions or selective avoidances of such severity that it may starve itself into a near-fatal cachexia rather than take readily available food in or

* Mowrer, O. H. Animal Studies in the Geneses of Personality. Tr. New York Acad. Sci. Ser. II, 3 (1). 1941.

out of the experimental situation. Again, it exhibits every physiologic evidence of anxiety in sensorial fields associated with the experimentally induced conflict. For instance, it shows sudden startled reactions, tachycardia and respiratory disturbances, when subjected to light or sound stimuli reminiscent of the feeding signals, or it becomes claustrophobically panic-stricken, if constricted into small spaces. Similarly, it may develop peculiarly stereotyped compulsive and counterphobic patterns, such as starting with blank immobility into the food-box without appearing to notice the food, or passing barriers aimlessly in a ritualized, stereotyped fashion. If more severely neurotic, it may become subject to various motor disturbances such as tics, repetitive movements, regressions to earlier forms of behavior, or even cataleptic rigidity. Concurrently, it loses dominance in its group hierarchy, while its relationships to the experimenter become altered either toward extreme dependence or aggressive hostility.

The Biodynamics of Therapy

The experimental methods that were found to be effective in alleviating these neurotic aberrations of behavior in our animals likewise seemed heuristically significant, in that they were operationally successful only in so far as they relieved the motivational conflict basic to the neurosis. Since there were seven of these "therapeutic" methods, and I am scheduled tonight to devote particular attention to the last of them—the use of alcohol and other drugs—I shall not presume upon your time by dwelling at length on all seven; however, perhaps their mere enumeration may serve to point out how closely they correspond to the techniques employed in the clinical therapy of human neuroses. Briefly, the methods are these:

First, providing the animal with a prolonged *vacation* from the conflict-producing situation amid relatively restful surroundings. This method, however, is slow, uncertain and does not prevent recurrences of neurotic behavior when the animal is again exposed to conflict. Incidentally, the author, his home and his domestic tranquility have suffered considerably from attempts to provide a rest-haven for neurotic animals.

Second, affording the animal some physiologic *relief from one of the conflictful drives.* For instance, if the hungry neurotic animal is fed before replacing it in the experimental situation, its anxiety and

phobic reactions are considerably diminished.

Third, the utilization of what might be called *transference relationships* to guide the animal out of its motivational impassé. Thus, an experimenter, whom a neurotic animal had previously learned to trust, may retrain it in gentle stages to re-explore the feeding situation, once again respond to signals and, finally, even withstand the conflict-engendering trauma. Conversely, if the animal's confidence in the experimenter is not first re-established, the retraining is difficult and ineffective.

Fourth, the use of some *environmental press* which, for instance, inexorably forces a starving neurotic animal to eat, despite its phobias and inhibitions. Again, this method is successful only in mild neuroses or if the animal is exceptionally well-integrated; in other cases, the animal may be driven into an almost uncontrollable panic.

Fifth, the employment of *"social example."* In this technique, a neurotic animal is placed in the cage with one that is responding normally to the feeding signals. In such circumstances, some neurotic animals may slowly overcome their own inhibitions and begin to emulate the behavior of their normal partners; at first, when they are together, and finally, when they are apart.

Sixth, and most effective, the *spontaneous working-through of the conflict-situation.* An animal that has learned the use of the switch and and is then made neurotic will, for a time, show a peculiar amnesia for the switch-manipulating pattern. However, as its hunger mounts from day to day, the animal may tentatively re-explore the use of the switch to secure food at optimum moments; and, in this way, gradually utilize what control it had previously attained over its environment to "work through" and establish anew normally goal-directed patterns of behavior. Once this is done, moreover, the animal is relatively immune to further traumata of a similar nature.

The seventh and final mode of therapy is the *administration of drugs that impair apperceptive discriminations and thereby diminish the acuteness of the adaptational conflict.* This is the method that I shall discuss later in particular connection with the effects of alcohol and their relationships to the dynamics of experimental neuroses. Before doing so, however, it may be well to summarize the significance of the experimental data so far presented and to correlate them with everyday clinical observations of what Adolf Meyer has well called "the experiments of nature in human behavior." May I, then, submit

to you four biodynamic generalizations which, I believe, apply to human beings as well as experimental animals, and which will be elaborated in my book, "Principles of Dynamic Psychiatry," now nearing completion? Each canon, of course, has numerous corollaries and qualifying codicils; but, in briefest statement, they are these:

The Biodynamic Principles of Behavior

1. Behavior is motivated by the physiologic needs of the organism.
2. The animal responds, not to "objective" reality, but to its own experientially determined *interpretations* of its milieu.
3. Under conditions of frustration, behavior becomes deviant and substitutive or "symbolic."
4. In circumstances that produce motivational conflict, behavior becomes ambivalent, inhibited, vacillating, stereotyped and excessively substitutive and symbolic—or, in clinical terms, "neurotic" or "psychotic."

* * * * *

This, then, is an exceedingly condensed survey of a decade of experimental work with hundreds of animals, and of what I believe to be the biodynamic integrations of our results with clinical and psychoanalytic observations of human behavior. On the one hand, I fear that my summary may have been telescoped beyond the point of adequate clarity and coherence. On the other, it might seem a rather elaborate prologue for the special studies on alcohol, were it not for two saving considerations: first, that it provided the rationale and the necessary central observations for these studies, and, second, that their presentation is to be mainly in the form of a motion picture, which, I hope, will vivify the actual experiments and their results. It remains, then, only to outline and, for purposes of accuracy, to qualify statistically what you will see illustrated.

Neurosis and Alcohol

Technique of the Experiments. As already described, a cat is trained in the experimental apparatus to pass various barriers and manipulate a signal switch preparatory to opening a box for the food reward. The effects of graded doses of alcohol on these normal adaptive patterns is then studied, after which the animal is made neurotic by a motivational conflict and again given alcohol. Finally, it is

permitted free access to alcohol to see if a spontaneous preference for the drug develops.

Results: Briefly, the results are these. When an animal is given from 0.7 to 2.0 cc. per kilo of alcohol, its most recently acquired and complex discrimination-response patterns (e.g., difficult switch-manipulations) are progressively disorganized until only the most primitive and elemental reactions remain. Conversely, as the animal emerges from alcoholic stupor, its adaptive capacities gradually become reconstituted until the highest orders of learned behavior again appear.

If now the animal is made neurotic and once more given alcohol in small doses of 0.5 to 1.2 cc. per kilo, essentially the same effects appear, but in an interestingly deviated form of expression. Thus, the inhibitions, phobias, loss of group dominance, compulsions, and other aberrant behavior patterns—i.e., those which constitute the most complex reactions to the conflict-producing situation—are again the ones first disintegrated, leaving the earlier signal-response and feeding patterns relatively intact. In our experiments, then, small doses of alcohol, by impairing discrimination and hypersensitivity of reaction, seemed to have an actually beneficial effect on neurotic behavior, although, of course, the inhibitions and other neurotic symptoms returned as the effects of the drug wore off. But here is where still another unexpected phenomenon appeared: twelve of the twenty-one animals which had experienced temporary relief of neuroses under alcohol, and which had previously refused to take alcohol spontaneously, now began to prefer food containing alcohol and thus, to all intents and purposes, became alcoholic "addicts." Interestingly, too, this preference rapidly abated as their neurosis was relieved either through spontaneous re-exploration of the conflict situation while they were intoxicated, or by the use of vacations, environmental press, retraining, or one of the other therapeutic methods previously described.

Effects of Alcohol in Mitigating the Neurotogenic Effects of Motivational Conflicts

The next question that naturally arose was: if alcohol, by impairing the acuity of discrimination, disorganizes neurotic behavior patterns, should not the drug also protect the animal from the effects of the conflictful experience itself? To this question, our experiments

have not yet given an unequivocal answer, and I can only report our results to date. Of nine cats given 0.5 to 1.5 cc. per kilo of alcohol and then subjected to an air-blast or electric shock during conditional food-taking, only four have reacted with the development of definitely neurotic symptoms, and, even in these, the behavioral aberrations have been either evanescent or considerably milder than in previous control animals not given alcohol. In contrast, the five other animals have shown a curious bravado and a remarkable immunity to motivational conflicts while intoxicated. For instance, three of them seemed relatively undisturbed by electric shocks strong enough to produce myoclonus, and two of them, instead of running in panic from the air-blast, would actually approach, sniff and play with the pneumatic tube. However, these few observations cannot yet be taken as determinative, since not only must a larger series of animals be investigated, but each one must later be made to serve as its own control by being subjected to the traumatic situation when not intoxicated. If then they develop definite neuroses, the experiments will assume significance and definite conclusions can be drawn.

Permit me now, before proceeding to a final brief discussion of these findings, to illustrate the work on experimental neuroses and on alcohol by the showing of a motion picture film that may, at last, give some substances to this discourse.

(Showing of motion picture film "Neurosis and Alcohol."*)

Discussion

Naturally, I recognize the vast differences between the most intelligent of alcoholic cats and the humblest of human topers. It seems hardly necessary to point out here that—even though the human being has a far greater capacity for complex perceptions, symbolizations and integrations, and a far larger repertoire of normal and neurotic adaptations—from an objective scientific viewpoint, he is still an animal whose behavior conforms to general biologic and biodynamic principles. It is these general principles that our work attempts, however remotely, to approach. And so we can, with considerable relevance, point out that the human being, too (as, for instance, a combat flier after a sortie), often resorts to alcohol to diminish the anticipated poignancy of a conflictful situation, and that, from a psychiatric standpoint,

* Prepared by the Division of Psychiatry, University of Chicago; distributed by the Psychological Cinema Register, State College, Pa.

chronic alcoholic addiction is very often a symptomatic escape from underlying neurotic anxiety. True, dipsomania in the human may have complex social, libidinal and regressive connotations not present in the elemental "unconscious" of the cat, but these higher symbolizations neither negate nor conflict with the phenomena dealt with in our experiments and may, in the last analysis, be themselves reduceable to simpler biodynamic terms.*

Summary

Certain biodynamic principles governing the motivations, conditions, expressions, and vicissitudes of behavior may be derived both from animal experimentation and from the study of human conduct. Of special interest to the present discussion is the principle that neurotic behavior appears when motivations are counterposed so that their corresponding adaptive patterns became conflictful. Alcohol, in moderate doses, disorganizes complex, recently learned response-patterns, whether normal or neurotic, and thereby releases earlier and more elemental forms of goal-directed behavior. These ameliorative effects of the drug are sometimes sought by animals that have experienced them, and may, in such instances, be of significance in the biodynamic and clinical relationships bewteen neuroses and alcoholism.

* For a documental discussion of this subject, see **Masserman, J.** Neuroses and Alcohol, Amer. J. Psychiat., Jan. 1945.

REPORT OF THE ANNUAL MEETING

DECEMBER 14, 1944

The 127th Annual Meeting of the Academy for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at The American Museum of Natural History on the evening of Thursday, December 14.

The Corresponding Secretary reported that there are now 45 Honorary Members upon the rolls of the Academy. The deaths of five Honorary Members and one Corresponding Member were reported during the past year.

The Recording Secretary's report stated that the Academy has held 30 Regular Sectional Meetings at which 45 stated papers of high scientific caliber were presented. In addition, two excellent addresses were delivered on the occasion of the Annual Business Meeting.

Eighteen informal receptions were held under the auspices of the various Sections of the Academy.

Three two-day conferences, on special subjects of research, were held during the year and were attended by outstanding investigators in these fields. The titles of the conferences are as follows: Section of Biology, "Animal Colony Maintenance"; Section of Physics and Chemistry, "Energy Relationships in Enzyme Reactions," "The Diffusion of Electrolytes and Macromolecules in Solution."

Six Honorary Life Members, 2 Life Members, 5 Sustaining Members, 129 Active Members, 56 Associate Members and 6 Student Members were added to the rolls, of which 200 are now in good standing and 4 await qualification through payment of dues. Thus, a total of 204 new Members was added during the year. Two Sustaining Members were transferred to Life Membership, having paid dues for twenty-five years. There are at present upon the rolls of the Academy 2,076 Members, of whom 311 are Fellows, classified as follows: 1 Patron, 92 Life Members, 122 Sustaining Members, 1,163 Active Members, 622 Associate Members, 31 Student Members, 45 Honorary Life Members.

The Editor reported that, since the last Annual Meeting, The New York Academy of Sciences published 1,022 pages to date. Of this amount, 474 pages are published in the Annals, 202 pages in the Porto Rico Survey and 346 pages in the Transactions. In addition to the

articles already published, Articles 1 and 2 of Volume 46 are in the hands of the Editor and are being prepared for the printer.

The detailed list of the publications issued during the year is as follows:

ANNALS

Volume 44, Article 6—"Psychosomatic Disturbances in Relation to Personnel Selection" by Lawrence K. Frank, M. R. Harrower-Erickson, Lawrence S. Kubie, Gardner Murphy, Donal Sheehan, and Harold G. Wolff. Pages 539–624. Published December 22, 1943.

This completes Volume 44

Volume 45, Article 2—"Lycaenidae of the Antilles," by William P. Comstock and E. Irving Huntington. Pages 49–130. Published December 29, 1943.
Volume 45, Article 3—"New Methods in Stellar Dynamics," by S. Chandrasekhar. Pages 131–162. Published December 31, 1943.
Volume 45, Article 4—"Studies on Fresh-Water Bryozoa. XIV. The Occurrence of *Stolella indica* in North America," by Mary D. Rogick. Pages 163–178. Published December 31, 1943.
Volume 45, Article 5—"The Social Behavior of the Laughing Gull," by G. K. Noble and M. Wurm. Pages 179–220. Published December 31, 1943.
Volume 45, Article 6—"Experimental Modification and Control of Molts and Changes of Coat-Color in Weasels by Controlled Lighting," by Thomas H. Bissonnette and Earl E. Bailey. Pages 221–260. Published April 7, 1944.
Volume 45, Article 7—"The Distribution of the Salamanders of the Genus *Plethodon* in Eastern United States and Canada," by Arnold B. Grobman. Pages 261–316. Published May 9, 1944.
Volume 45, Article 8—"The Organization of The New York Academy of Sciences." Revised to August 1, 1944 by Eunice Thomas Miner, Executive Secretary. Pages 317–356. Published September 7, 1944.
Volume 45, Article 9—"Energy Relationships in Enzyme Reactions," by Joseph S. Fruton, Eric G. Ball, Max Bergmann, Herman M. Kalcker, Otto Meyerhof and Carl V. Smythe. Pages 357–436. Published November 10, 1944.

This completes Volume 45

PORTO RICO SURVEY

Volume 12, Part 4—"Insects of Porto Rico and the Virgin Islands—Rhopalocera or Butterflies," by William P. Comstock. Pages 421–622. Published October 12, 1944.

TRANSACTIONS

Series II, Volume 6, Nos. 1–8, consisting of 346 pages, was completed, printed and distributed each month from November, 1943 to June, 1944 inclusive.

The Librarian reported that, the Academy has distributed, in response to requests and orders, 38,993 separate publications and 2,169 complete volumes of Transactions, Series II, as follows: *Annals*, 32,943; *Scientific Survey of Porto Rico and the Virgin Islands*, 891; *Special Publications*, Volume I, "Climate and Evolution," 121; Volume II, "Balinese Character," 251; and *Transactions*, Series II, Volume 5, com-

plete volumes, 2,169, and 4,787 single copies.

The Library of the Academy has received from exchange institutions, 1,295 separate publications.

The Treasurer reported that the Academy has just completed the most successful financial year in its history, in its receipts from membership support and sales of publications.

The total income received from all sources amounted to $26,626.53. This is an increase of $1,197.37 above that of 1943.

The Academy has added 204 new names to its rolls thus adding a gain of $900.30 to our revenue from membership dues, making a total of $8,745.30 from this source.

Receipts from sales of publications amounted to $8,253.66, an increase of $2,830.50 over that of last year.

The income from investments, amounting to $3,696.13 represents a yield slightly less than 4.6%.

A contribution of $1,500 was received from Mr. William Otis Sweet, a Sustaining Member of the Academy, who requested the establishment of the William Otis Sweet Fund, the exact definition of which is to be made later.

Included in the debit balance of $1,905.30 of the Surplus Account is $633.29 carried over from 1943; $1,001.93 covering advance payments on publications which will be charged against publication funds in 1945; and, $270.08, the balance remaining when all further payments on account were terminated by the Bank of United States in Liquidation. This latter amount was written off the Academy's books by Council authority.

The book value of the Academy's property, as of November 30, 1944, now amounts to $84,878.61; of which $76,615.89 is in stocks and bonds; $867.25 is a mortgage bond; and the remainder, $6,567.35 is represented by cash in bank, a portion of which is awaiting reinvestment.

The books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1944, and have been audited by the Finance Committee, as provided by the Constitution and By-Laws.

The property of the Academy was verified and the Treasurer's report examined and attested by the Finance Committee, as of December 6, 1944.

The A. Cressy Morrison Prizes of two hundred dollars each, for the

two most acceptable papers in any field of natural science, within the scope of the Academy and its Affiliated Societies, were awarded to the following papers, entitled:

"The Effect of Activity on the Latent Period of Muscular Contraction," by Alexander Sandow, Department of Biology, New York University, New York, New York.

"A Hitherto Undemonstrated Zoogleal Form of *Mycobacterium tuberculosis*," by Eleanor Alexander-Jackson, Department of Public Health and Preventive Medicine, Cornell University Medical College, New York, New York.

The two following papers, because of their general excellence, were awarded Honorable Mention by the Committee of Judges:

"Respiration and Germination Studies of Seeds in Moist Storage," by Lela V. Barton, Boyce Thompson Institute for Plant Research, Yonkers, New York.

"An Early Site in Cayuga County, New York; Type Station of the Frontenac Focus, Archaic Pattern," by William A. Ritchie, Rochester Museum of Arts and Sciences, Rochester, New York.

The Prize Committee begs to congratulate the authors of these papers, on behalf of the Academy.

The Committee takes great pleasure in announcing that Mr. Morrison has renewed his offer of two additional prizes in Natural Science, of $200 each, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society, to be awarded in December, 1945. The terms of competition will be published in the next issue of the *Transactions*.

The following members were elected to Fellowship:

Theodore Mead Abel, Ph.D.
Jerome Alexander, M.S.
M. L. Anson, Ph.D.
Conrad Arensberg, Ph.D.
Harriet Babcock, Ph.D.
Boris A. Bakhmeteff, C.E., D.E.
R. Bowling Barnes, Ph.D.
Charles H. Behre, Jr., Ph.D.
William C. Boyd, Ph.D.
Arthur M. Buswell, Ph.D.
Eben J. Carey, Sc.D., M.D.
Edward H. Cox, D.Sc.
H. Jermain Creighton, Sc.D.
John R. Dunning, Ph.D.
Isidore Fankuchen, Ph.D.
Joseph S. Fruton, Ph.D.
Lester H. Germer, Ph.D.
E. Newton Harvey, Ph.D.
Ethel Browne Harvey, Ph.D
Elvin A. Kabat, Ph.D.
Vladimir Karapetoff, D.Mus., Sc.D.
George E. Kimball, Ph.D.
Edwin Kirk, Ph.D
Maurice E. Krahl, Ph.D.
George Lawton, Ph.D.
Otto Marburg, M.D.
Herman F. Mark, Ph.D.
Valy Menkin, M.D.
Howard C. Moloy, M.D.
Hans Neurath, Ph.D.
Willard Z. Park, Ph.D.
Edith H. Quimby, Sc.D.
Marie Reimer, Ph.D.
Robert Simha, Ph.D.
Maude Slye, Sc.D.
H. B. Van Dyke, Ph.D., M.D.

Honorary Life Membership was conferred upon the following eminent scientists:

Robert H. Lowie, Anthropology. Professor of Anthropology, University of California, Berkeley, California. Ph.D., Columbia University, 1908; Sc.D., University of Chicago, 1941.

Paul Niggli, Mineralogy, Petrography. Professor, Mineralogy, Zurich University and Federal Institute of Technology. Diploma in Natural Sciences; Ph.D., University of Zurich; Dr. Eng. (H.C.), Ehrenbürger technische folkschule, Carlsruhe, Zurich, 1933; D.Sc. (H.C.), Technische folkschule, Stuttgart, 1933; D.Sc. (H.C.), University of Geneva, 1935; D.Sc. (H.C.), University of Budapest. Research Fellow, Carnegie Institution of Washington. Awarded Silver Medal in Sciences, Federal Institute of Technology; President, Geotechnical Commission of the Swiss Society for Natural Science Research.

Florence Sabin, Anatomy, Pathology. Member, Rockefeller Institute for Medical Research, 1925–38, Emeritus Member, 1938–, New York, N.Y. Sc.D., Smith College, 1910; M.D., Johns Hopkins Medical School, 1900; Sc.D., University of Michigan, 1926; Mt. Holyoke College, 1929; New York University, 1933; Wilson College, 1933; University of Syracuse, 1934; Oglethorpe University, 1935; University of Colorado, 1935; University of Pennsylvania, 1937; Oberlin College, 1937; Russell Sage College, 1938; LL.D., Goucher College, 1931. Awarded National Achievement Award, 1932 and M. Cary Thomas Prize, Bryn Mawr, 1935.

The following officers were elected for the year 1945:

For President
WALTER H. BUCHER

For Vice-Presidents

MARSHALL KAY — RAYMUND L. ZWEMER
ANNE ROE — JOSEPH S. FRUTON
HORTENSE POWDERMAKER — RAYMOND B. MONTGOMERY

For Recording Secretary
MICHAEL HEIDELBERGER

For Corresponding Secretary
H. HERBERT JOHNSON

For Treasurer
DONALD BELCHER

For Librarian
BARNUM BROWN

For Editor
ROY W. MINER

For Councilors (1945–1947)
RALPH H. CHENEY
ROBERT CUSHMAN MURPHY
HERBERT F. SCHWARZ

For Finance Committee
HARDEN F. TAYLOR, *Chairman*
HARRY B. VAN DYKE
ADDISON WEBB

After the Business Meeting, the following address was delivered by the speaker of the evening:

"Polymers and Light"

by

PETER DEBYE

Professor of Chemistry, Cornell University

(Illustrated)

Polymers are giant molecules, although in general still too small to be seen with an ordinary microscope. Their unusual size or shape together with a peculiar flexible structure, which many of them possess, are mainly responsible for outstanding technical properties of the finished product (Example: Rubber). When light passes through a solution of such polymers a small part is scattered analogous to the part of the sunlight scattered in our atmosphere, giving us the impression of a blue sky. The address deals with the answer to the question as to how measurements on the strength and distribution of the scattered light from polymer solutions can be used to determine size and shape of these invisible particles.

(Abstract by the Speaker)

ALEXANDER SANDOW, Department of Biology, Washington Square College of Arts and Science, New York University: *The Effect of Activity on the Latent Period of Muscular Contraction.** (A. Cressy Morrison Prize Winner, 1944.) (Abstract.)

This research deals with the effects of a muscle's activity on the various features of that part of the contractile response known as the mechanical latent period—the time interval between the instant of application of a stimulus to the muscle fibers and the instant at which the first sign of tension development appears. In frog skeletal muscle, this interval lasts about 3 ms. Since a burst of contractile activity of a muscle not only alters the latency behavior in subsequent contractions, but also causes marked and quite well-known changes in the muscle's internal chemical milieu, the possibility arises of attempting to elucidate the nature of the latent period in the light of the correlation between the activity-induced latency and chemical effects.

The latest period is not, as has been generally thought, a time of complete mechanical quiescence. It was first demonstrated by Rauh, in 1922, that a frog skeletal muscle actually relaxes very slightly during the latter part of its latent period, just prior to the development of tension. This "latency relaxation" (abbreviated LR) involves a maximum negative tension change in frog sartorii of 10–20 mg., i.e., only about 0.05% of the positive tension at the peak of a twitch. This is so minute—the equivalent LR increase in muscle length being only about 0.1 μ—that even the most sensitive mechano-optical muscle registering levers can hardly do more than detect the change. It has therefore been necessary to devise for the present research a new, electronic, method for recording the LR. This involves the conversion of the latency mechanical events into an electric pulse by means of a piezoelectric pickup (actually the working unit of a crystal phonograph pickup) and the amplification of this pulse so as to actuate a cathode-ray oscillograph. The apparatus is in effect an electronic lever which, with a magnification factor of about 500,000× presents the minute LR pre-contractile elongation as a deflection of some 5 cm. on the cathode-ray screen. Measurement of photographs of such deflections permit LR alterations to be determined with a precision of $\pm$ 0.1 mg. tension, or equivalent $\pm$ 0.002 μ length, change. A time calibration

* Research supported in part by a grant from the Penrose Fund of the American Philosophical Society.

impressed on each latency record enables time intervals to be read with a precision of ± 0 02 ms.

All the experiments of the present study have been done on frog sartorii suspended, under an initial tension of about 3 gm., in the moist air of a muscle chamber at temperatures of from 22° to 25° C. The muscles have been activated by either a tetanus, a series of tetani, a series of twitches, or certain combinations of these, and the corresponding latency changes determined.

It has been found that activity in any amount, even a single twitch, causes an immediate reduction in R, the depth of the LR. The greater the extent of the activity, the greater is the decrease in R, so that, after a series of, say, fifteen 2 sec. tetani, R is reduced to about 10% of its pre-activity value. The time interval between the instants of stimulation and of the beginning of the latency relaxation, L_R, is not affected by activity unless the muscle has been brought to a stage of advanced fatigue, when L_R tends to increase.

The results concerning the latency for positive tension development —the true mechanical latent period—, L_T, are more complex. (Actually, L_T consists of a set of three latencies, each measured from the instant of stimulation to a particular instant within the time during which the LR develops and is reversed by the earliest increments of tension rise. These L_T's, however, always vary in parallel and, for the present needs, it is not necessary to distinguish between them. L_T will therefore be used here to symbolize the duration of the mechanical latent period.) Due to slight activity, 5 or 6 successive twitches at 3 sec. intervals, L_T increases by at most 0.2 to 0.3 ms; further activity, amounting to 4 or 5 successive 2 sec. tetani, then causes L_T to decrease by as much as 0.5 ms.; any still further activity results in increases in L_T up to or even beyond the original pre-activity values. All of the activity changes listed above are, in general, reversed by rest.

It is known that the LR is a function of myosin, the contractile protein, and the present research proves, furthermore, that the changes in the latency variables caused by activity are not due to effects on the excitatory aspects of the response, which, if they occurred, might conceivably be reflected in contractile modifications, but are due to the direct effect of the activity on some feature of the contractile complex of the muscle.

In searching for the particular contractile feature that is involved, the interpretation of the results concerning R and L_R have not been

very helpful. The L_T results, however, are very suggestive. The changes in L_T, due to activity and subsequent rest, show a striking correlation with the course of the pH changes that other workers have demonstrated in frog muscles activated similarly to those in our experiments. The general conclusion derivable from this correlation is that the tension latency is shorter, the higher the pH of the muscle. It thus follows that the rate of some process that is directly involved in determining the speed of tension rise is greater, the more alkaline the muscle. Since the hydrolysis of adenosine-triphosphate (ATP), undoubtedly an important source of energy for muscular processes, is known to increase in rate with increase in pH (up to about pH 9.0), and since other relevant muscle reactions do not have this property, it is inferred that the hydrolysis of ATP is the process that directly determines the rate of rise of tension. This inference thus indicates that the energization of myosin, the contractile protein, occurs during the activation (i.e., latent and contraction) periods of a muscular response, and not, as some believe, during the recovery phases of the response. Furthermore, this inference, and some other implications of the data, are shown to be in accord with a particular conception of the energizing mechanism in muscle previously proposed by the author, that the LR is a mechanical sign of a tension-induction process involving coupling in the form of an enzyme-substrate combination of myosin and ATP during the existence of which the energy of the first phosphate bond of the ATP is transferred to the myosin, thus energizing it and activating it for contraction.

Eleanor Alexander-Jackson, Department of Public Health and Preventive Medicine, Cornell University Medical College: *A Hitherto Undemonstrated Zoogleal Form of Mycobacterium Tuberculosis.* (A. Cressy Morrison Prize Winner, 1944.) (Abstract.)

The existence of non-acid-fast forms of *Mycobacterium tuberculosis* has been recognized at least as far back as 1900, but the reproductive role played by them has been variously interpreted by many observers. At the present time, there are four main schools of thought.

The first school regards non-acid-fast rods and granules as degenerative in nature since they are to be found in old cultures, and the supporters of this view claim that they were unable to observe any mode of reproduction other than binary fission.

The second school includes those who believe in the filterability of certain elements of this organism.

The third school includes those who, while unable to demonstrate filterable forms, are of the opinion that the non-acid-fast rods and granules are in many instances young organisms which have not as yet developed acid-fastness, but later on develop into acid-fast rods. The adherents of this latter group agree that binary fission may be the common method of reproduction, but find that non-acid-fast granules, obtained through segmentation, either elongate or sprout into the rod forms of *Mycobacterium tuberculosis.*

Finally, there is a group that regards non-acid-fast forms, in many instances, as artifacts brought into being by injury suffered by the microtome knife or by manipulative damage with the platinum loop in preparing slides for staining. The conclusions drawn from their work are vitiated by the fact that the authors describe a technic so different from those usually employed in producing stained preparations that no comparison can be made.

In this paper, the author demonstrates that:

Mycobacterium tuberculosis can exist not only as rods or granules, but also as a zoogleal plasmodium consisting of granules or larger globoid bodies surrounded or enmeshed by amorphous material.

These zoogleal forms are not revealed by the usual stain technics (unless acid-fast). They are made observable by a new staining technic described by the author.

Zoogleal forms have been observed repeatedly in unstained material, as well as in preparations stained by the triple stain method.

Single-cell studies and electron microscope photographs of matetrial from pure cultures, indicate that zoogleal forms, under suitable environmental conditions, are able to revert to rod forms and *vice versa*.

It has been observed that diphtheoids are also able to enter a zoogleal state, and to revert to rod forms.

The demonstration of forms which are not revealed by the usual technic, may throw some light on the question of the apparent disappearance of acid-fast bacilli or their paucity during the course of some tuberculous infections.

Much additional work with both pure cultures and clinical material is necessary to understand more fully the part played by the zoogleal forms in tuberculous infection. We still do not know just what happens to the organisms when acid-fast bacilli disappear from sputum in arrested cases, only to reappear in large numbers with the return of active disease. Are most of them destroyed, or does a portion of them undergo a dissociative change in response to an environment temporarily unfavorable to the maintenance of the characteristic rod form of *Mycobacterium tuberculosis?* Another mystery is the difficulty, at times, of finding any acid-fast bacilli in stained preparations from pathological material where one would expect to find many. Frequently a definite diagnosis of tuberculosis is delayed by failure to find typical red-staining rods until some time after the first laboratory examination.

In approaching these problems, the writer is only too well aware of the great difficulties which lie along the path, a path made thorny by the ever present danger of confusing the "real thing" with non-tuberculous elements—in particular, the diphtheroids. The utmost care, therefore, was taken to exclude their presence, and to maintain a stoic skepticism until repeated close observations had been made. The work described above covers a ten-year period.

LELA V. BARTON, Boyce Thompson Institute for Plant Research, Inc.: *Respiration and Germination Studies of Seeds in Moist Storage."* (Honorable Mention, A. Cressy Morrison Prize Competition, 1944.) (Abstract.)

There are many published reports of seeds that were supposed to have been buried in the soil for decades and yet remained dormant and viable. Most of these accounts have been based on the appearance of plants not common to the region on newly excavated or plowed soil. Similar observations have been made recently for bombed areas in Great Britain. Any gardener will testify to the continued reappearance of weeds in his garden in spite of careful elimination of the seedlings as they appear year after year. The results of controlled seed burial tests conducted by several workers have confirmed these general conclusions. This means that the soil is stocked with seeds which are capable of germination when they are disturbed. Some of these seeds which possess hard coats finally germinate when fungi and bacteria or low temperature have caused the deterioration or the cracking of the impermeable part of the coat. Many other seeds, especially weeds, which do not have hard coats remain fully imbibed in the soil for long periods. *Amaranthus* and *Rumex* are among the latter. They have been shown to remain viable under these conditions for forty years. Their dormancy or failure to germinate must be due to a very delicate equilibrium, for it can be overcome by many factors, such as light, fluctuating temperatures of the top soil, mechanical disturbance, or oxygen supply. When seeds take up water preparatory to germination, their metabolic activities become greatly accelerated. It is evident that imbibed seeds could not remain viable very long in the soil unless definite curtailment of these activities took place. No actual measurement of this curtailment has ever been reported.

Respiration studies on seeds of *Amaranthus retroflexus* L., *Rumex obtusifolius* L., and *Impatiens balsamina* L., held moist and without germination, were conducted. This involved the development of a new technique to permit the measurement of the oxygen consumed and the carbon dioxide evolved by dormant seeds. Germination tests, the results of which demonstrated the delicate nature of the mechanism imposing such dormancy, are also included.

Gaseous exchange of *Amaranthus retroflexus* seeds, measured at intervals of from 0 to 901 days of moist storage at 20° C., showed at least a ten-fold reduction in respiration. The beginning of this reduc-

tion became apparent very early (after two days), and was definite after eight days in moist storage. Decreased respiration was also noted for *Impatiens balsamina* seeds held moist at 20° C. for 28 to 365 days. With increased length of time in moist storage, the respiratory quotient decreased.

Seeds of *Amaranthus retroflexus* held in moist storage showed a periodicity in germination which was apparently independent of external conditions. This indicated varying degrees of the primary dormancy or the induced secondary dormancy of the original lot of seeds. Moist *Amaranthus* seeds held without germination at 20° C. could be induced to germinate at that same temperature by rubbing, by drying for three hours to three days, or by exposure to 35° C. for 2 to 24 hours. Germination also proceeded immediately after removal to higher constant or alternating temperatures.

Moist seeds of *Rumex obtusifolius*, held without germination at 30° C., could be made to germinate at this same temperature by removal of the coats, treatment with concentrated sulphuric acid for two minutes, or exposure to 5° C. for four days. Upon removal from 30° C. to lower constant temperatures or daily alternating temperatures, germination proceeded without further treatment.

Many variable factors which doubtless affect the life span of seeds in soil were not present in the controlled experiments reported here. However, these studies indicate some of the fundamental changes which take place in seeds during moist storage and may serve as a beginning for further investigations.

WILLIAM A. RITCHIE, Rochester Museum of Arts and Sciences: *An Early Site in Cayuga County, New York; Type Station of the Frontenac Focus, Archaic Pattern.* (Honorable Mention, A. Cressy Morrison Prize Competition, 1944.) (Abstract.)

This paper constitutes the formal report of two seasons (1939–1940) of excavation work conducted by the writer on an archaeological site of exceptional richness and significance to New York prehistory. His previous researches in the state had defined two discrete archaeological horizons (Lamoka and Brewerton Laurentian) pertaining to a prehorticultural and preceramic time-level, designated the Archaic, with vague suggestions of a partial temporal overlap. At Frontenac Island, there appeared, for the first time, clear evidence of the contact of these different peoples and cultures, resulting in a composite new culture, termed the Frontenac Focus of the Archaic Pattern.

The materials found and presented for analysis included nearly 2,000 industrial artifacts (from midden and graves), 159 human burials, and numerous other features, as dog burials, hearths, pits, and simple stone structures.

Typological studies of the midden artifacts and site histograms to show their distribution revealed the occurrence of a superficial upper zone congeries, undifferentiated by physical stratigraphy, which pertained to a relatively little known culture termed the Point Peninsula Focus. The genetically unrelated culture of the much deeper lower zone proved, on analysis, to be composed very largely of the diagnostic and linked traits of the Lamoka (28.8%) and Brewerton Laurentian (25.8%) cultures, with the addition of 20.4% of novel traits. Detailed distributional studies showed for this complex no variation in typology as a function of depth; hence, the Archaic level of the site appeared to indicate, not a succession of the constituent cultures, but an amalgamation thereof, pertaining essentially to one time horizon.

The assessing of the mortuary data (belonging almost exclusively to the Archaic occupation) for their contribution to the interpretation of the site involved the ascertaining of correlations relative to (1) mode and order of burial; (2) craniological character of the restorable and mensurable skulls (45 in number); and (3) occurrence and typology of mortuary or grave goods. The following facts were established: (1) All modes of inhumation noted, viz., flexed, extended, crema-

tion, and bundle, were simultaneously practiced throughout the Archaic time range, as determined by multiple and superimposed burials. (2) Three cranial groups, which may be designated the dolichocranial (group 1), mesocranial (group 2), and brachycranial (group 3), were metrically and indically distinguished. The first is characteristic of the Lamoka Focus, the third of the Brewerton Laurentian Focus, the second seems to be the hybrid product of misecgenation. A morphological analysis of the entire series shows two distinct types: type 1, conforming to the Indian Knoll Sylvids; type 2, provisionally of the Younge type Sylvids (both typed by Neumann). Group 1 and type 1 correlate perfectly, as do group 3 and type 2. Both types occur in the mesocranial, group 2.

A moderate degree of correlation exists between physical type and burial mode. Type 1 skulls were associated with nearly twice as many flexed burials as type 2 skulls, and, conversely, type 2 crania occurred with one-third more extended skeletons than did type 1 crania.

Some correlation also is evident when burial mode, physical form, and typology of grave goods are considered. Grave goods of Lamoka culture were associated principally with flexed burials and type 1 crania, grave goods of Brewerton Laurentian type occurred almost exclusively with extended burials and type 2 crania, while grave goods combining deterministic traits of both the Lamoka and Brewerton Laurentian foci were found only with extended burials producing skulls of both morphological types.

Several alternative hypotheses were tested in the light of the complete analytical data, resulting in the following conclusions: The lower component at Frontenac Island represents the composite product of two discrete Archaic cultures of the New York area, Lamoka and Brewerton Laurentian, with their equally distinctive ethnic correlates; but, within this mixed population, cultural integration (as well as physical amalgamation) was not complete, old traits and customs still in part prevailing in combination. There seems also to have been some dominance of the Brewerton Laurentian complex over the simpler and probably older Lamoka paradigm.

Not only does the Frontenac Focus bridge the hiatus in our Archaic sequence, but it also discloses the earliest appearance of a number of constituent traits of the later Woodland and Hopewellian manifestations. It may also be compared profitably with the Shell-Mound Aspect of the Archaic Pattern of the southeastern United States.

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SECTION OF BIOLOGY[1]

JANUARY 8, 1945

DOCTOR T. C. BARNES, Associate Professor of Physiology, and DOCTOR R. BEUTNER, Professor of Pharmacology, Hahnemann Medical College and Hospital, Philadelphia, Pa.: *Electrical Pulsations in the Human Brain.*

(This lecture was illustrated by motion pictures.)

Electroencephalography is the study of electrical waves recorded through wires placed on the scalp. These waves are believed to come from the cerebral cortex, but deeper brain currents are known to spread upward to the electrodes as in subtentorial lesions. The normal frequency is 8 to 40 seconds and the normal voltage, 5 to 75 microvolts. The 10 per second waves are called *alpha;* slower, *delta;* and faster, *beta;* but they are the same waves at different speeds.

A one year old child has waves of less than 4 per second. Foetal brain waves have been recorded through the mother's abdominal wall and are similar to the central waves of the neonate. Electrical maturity is attained at the age of 12 in the visual or occipital regions. There is slight acceleration to the age of 18 in other areas. Brain waves of very aged patients are perfectly normal. Like Peter Pan, we never grow old. According to Golla, the occipital waves of women are faster than those of men, indicating that women are more keenly aware of their immediate surroundings.

[1] No meeting was held in January by the Section of Geology and Mineralogy.

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Some electroencephalographers claim that correlation exists between the frequency of brain waves and personality type. According to Saul and Davis, the fast type of brain wave indicates a dynamic personality; the slow type, a passive, dependent personality.

High alphas are statistically prone to asthma and ulcer. In schoolboys, what is considered a poor personality by the teacher is usually associated with slow waves. A high percentage of delinquent children have slow waves. Many physiologic conditions influence the brain waves, especially blood sugar. We have found a critical value of 130 mg. %. If a normal person has a blood sugar below this, hyperventilation will produce delta waves in most cases. Blood sugar should be taken with each electroencephalogram. The value, at the time of the test, is significant, for it indicates how much sugar is going to the brain. The sugar produces the energy of the brain and it may take place in the carbohydrate cycle yielding acetic acid, which combines with choline, forming acetylcholine.

We have produced artificial electrical brain waves by bringing acetylcholine into contact with brain extract. This work is a continuation of Dr. Beutner's life-long work on phase boundary potentials. This type of work has been completely neglected by neurophysiologists. For many years, oils, such as nitrobenzene, guaiacol, cresol, were used in the oil cell to test the electrical activity of drugs and hormones. In the last few weeks, we have succeeded in establishing a potential between acetylcholine and cholesterol. Cholesterol is an extract from the spinal cord, but cannot be used in its crystalline form. A solvent had to be found which, in itself, would give no electrical phase boundary with acetylcholine,—would dissolve cholesterol, conduct the current, have a density approximately that of water. After a long search, benzyl alcohol was found to satisfy these requirements, but it will not dissolve more than about 2% cholesterol. But this cholesterol gives a definite potential always negative with acetylcholine. It is known that activity of the brain waves probably arises from liberation of acetylcholine in the brain (stimulation of the petrosal nerve in the cat). Also the slowing of the waves during hyperventilation may be ascribed to the alkalosis of acapnia. It is well known that the hydroxyl ion hydrolyzes acetylcholine. During hyperventilation, the skin temperature also falls, indicating vasoconstriction with attendant cerebral ischemia.

In diagnosis the electroencephalogram is a valuable aid in the

localization of cerebral tumors. At the Massachusetts General Hospital, their records show an accuracy of 85% in localization of cerebral tumors by the e.e.g. The e.e.g. is also very useful in the study of epileptics. In head injury, the e.e.g. will indicate if the brain has been damaged. In encephalitis, large smooth waves are found. The e.e.g. will also frequently detect malingering. Faked blindness and faked amnesia show normal e.e.g.s.

Stimulating drugs, like benzedrine and epinephrine, produce a slight increase in frequency, while pentothal and morphine slow the waves. Dilantin prevents the seizures of epilepsy and destroys the epileptiform waves. Luminal, on the other hand, has little effect on the e.e.g.

The e.e.g. is of value in our war effort. Even evacuation hospitals are now supplied with electroencephalographs. They are useful in determining when a serviceman with head injury can return to duty. In one case, a young sailor was found asleep at his battle station. The e.e.g. showed that he had sleeping sickness and so he was honorably discharged. At induction centers, the e.e.g., is useful in borderline cases, more so than in civilian life, owing to the attempts of some candidates trying to hide epilepsy and of others to fake pathologic conditions.

SECTION OF PSYCHOLOGY

JANUARY 15, 1945

DOCTOR CLARK L. HULL, Institute of Human Relations, Yale University, New Haven, Conn.: *Moral Values, Behaviorism, and the World Crisis.*

Whatever else it may be, human conflict involves disagreements, and these disagreements, at bottom, concern values in general and moral values in particular. The present armed conflict and world crisis is a symptom, at least, of a moral and ethical breakdown on a huge scale. Why is it that men of various civilized cultures can agree on matters of pure science but not on concrete moral issues? The attempt to understand this great paradox naturally leads us to go back and reëxamine our basic moral and ethical principles, on the one hand, and our scientific procedures on the other.

At the outset of our examination of the problem, we encounter the question of the methodology according to which the investigation shall be conducted. The methodology of metaphysical speculation will be discarded at once, because of its historically demonstrated inability to mediate agreement among men. For similar reasons, religious dogmatism will also be passed by in our quest. Upon the whole, history shows that religion is about as likely to cause conflict as to prevent it. The methodology of pure science has, on the other hand, an excellent record of mediating agreement among the scientists of all civilized nations. For this reason, many people have felt that we should reëxamine the methodology of pure science, to see what possibilities it may offer for understanding the nature of this age-old problem.

It is at once clear that the problem before us concerns principles or theory, rather than fact. Accordingly, we turn for guidance in methodology first to physical theory or mathematical physics as the portion of scientific theory at present most perfectly developed. But here we find a world of jumping electrons, on the one hand, and of thermodynamic equations, on the other. It is a little hard to see how an electron could, as such, have any moral obligation to jump in one direction rather than another. Similarly, it is equally hard to attach any moral significance, as such, to the flowing of heat from a point of high to a point of low temperature.

And so we might pass through the sciences, examining them one by one for light on the problem of moral conduct. It is evident that, somewhere in this hierarchy of the sciences, we encounter the phenomena of value in general and of moral value in particular. These phenomena will hardly be found in the theory of heavy water, for example, but they surely appear somewhere among the biological or behavioral sciences, certainly below the level of cultural anthropology. It is my opinion that ordinary food or "gut" values (incipient economic valuative phenomena) have clearly appeared at the level of lower mammalian behavior (e.g., the albino rat), and that moral valuative phenomena have definitely appeared at the level of normal human behavior and possibly, to a small degree, among the higher anthropoids.

Since economic values are theoretically considerably simpler and less charged with prejudice than are moral values, let us glance at them first. Within the last twenty years the more important basic molar laws whereby organisms come to value, i.e., strive for, certain objects, substances, or conditions, have gradually become fairly clear. In general, any act which is performed shortly before the reduction of a primary need, like that concerned with food, water, pain, optimal temperature, or sex, will be conditioned in such a way that when the organism is again in that situation or one resembling it, and suffers from that need or one resembling it, that act will tend to be evoked. This seems to be the basic molar law of conditioning or learning.

After habits have been set up in this manner, the organism, when suitably motivated, will exert itself, i.e., will strive, not only to attain food but to attain a state of affairs which will *lead* to food either directly or indirectly. The consumption of physiological energy in the pursuit of such goals or ends may accordingly be characterized as *work* or *striving*. Thus, generally speaking, that may be said to be valued which is striven for and, other things being equal, the maximum amount of work which an organism will execute to attain a given reinforcing state of affairs may be taken as an indication of the valuation of that state of affairs by that organism. Here, then, we have the basis, not only for an experimental science of value, but also for a theoretical science of value.

In this connection, there may be recalled the relationship mentioned a little while ago between value theory and scientific theory. For this reason, I shall pause briefly to remind you of the characteristic methodology of scientific theory.

1. Scientific theory sets out with a set (a) of concepts, and (b) of nciples or hypotheses derived in one way or another from observa- ı of one kind or another.

2. These are organized into a set of definitions and postulates; the ‚er, especially, in an ideal development, taking the form of equations.

3. If, then, a given set of observable dynamic conditions is as- ıed, and the principles of the system really apply to them, it is 'ely a matter of mathematical manipulation of the postulational ‚ations to tell what will be the outcome of the situation in question.

4. If, now, an actual dynamic situation such as was assumed is ountered, or is set up experimentally, and if it (the situation) un- ls, as the theory has implied, we may say that the theory has ac- red an increment of verification or substantiation and, in so far, is e. If, on the other hand, the empirical outcome is different from t predicted by the theory, the latter may be said, in so far, to be ıe. This comparison of deductive prediction with fact is known as *idation,* and is an absolutely indispensable part of pure science.

The procedure thus briefly outlined amounts to an operational defi- on of scientific theoretical truth.

It is quite clear from the above summary statement of scientific ‚hodology that there is no *a priori* impossibility of ultimately attain- a molar theory of organismic behavior which will cover all aspects ‚he striving of organisms. Moreover, it is equally clear that such a ory, when worked out, will be capable of being proved valid or in- id by the empirical test of observing what really happens, follow- the occurrence of any dynamic conditions to which the theory ılies.

Now, it is but a step from this to a theory of behavior which will ble us to predict, on the average, what *human* organisms will do ler all sorts of conditions. It should even be possible, ultimately, to dict the verbal reactions which people make, i.e., what they will *say* ırding their approval or disapproval of the behavior of others as l as their own. Thus, the methodology of science, presumably, will mately apply to moral behavior, even including the *moral judgment,* ıe this is regarded as a bit of verbal behavior concerning people's ròval or disapproval of *other* behavior.

But here we encounter a critical question, one concerning which 'e is a great deal of current confusion among both scientists and cists: Is the capacity of the methodology of pure science to mediate

the *prediction*, i.e., the logical deduction, of the occurrence under given conditions of behavior of whatever nature, whether moral, immoral, or neutral, the same thing as the capacity to *characterize* certain behavior absolutely as ethically good or bad?

As so often happens, the clear posing of a problem furnishes us with valuable clues to its solution. The clue in the case of the present problem is the distinction between *prediction* and *characterization*. No ethical system that I know about attempts to *predict* the occurrence of any event whatever.

Does this difference between ethical theory and the theory of moral behavior mean that ethical principles inherently can never have the type of validity that the scientific theory of moral behavior may have? I fear that the considerations just outlined leave us no alternative. So long as ethical theory only mediates the *characterization* of events if, or when, they occur, but never *predicts* the occurrence of anything, there can be no objective scientific test of its truth or falsity; i.e., there is no scientific means of determining its validity.

But statements which cannot be tested for truth or falsity cannot be said to be either true or false. This means that such statements occupy a scientific no man's land, which is practically equivalent to saying that such statements are scientific nonsense. This is, I think, the reason why men who are familiar with the techniques of science, by and large, are able, in the course of time, to attain substantial agreement in scientific matters, but as a rule make little progress toward agreement in regard to matters of moral conduct where serious concrete issues are involved. It follows that the so-called science of ethics, so far as *ultimate* ethical values are concerned, is a pseudo-science.* Meanwhile, this presents no impediment in the way of the development of a true science of moral behavior, including the moral judgment, because this is concerned with events which may be predicted and publicly observed. Neither does it impede the application of science in the determination of the most effective means of attaining values of all kinds, ethical or otherwise, as held by ourselves or others.

By much the same reasoning, it is concluded that the hope of somehow deriving ethical principles from the innate constitution of the "mind," on the analogy of the "self-evident" truths of logic and Euclid's approach to geometry, is also doomed to disappointment. This is be-

* It is to be noted that here the term *ethics* is employed in the technical sense of the alleged science of what absolutely *is* good or bad as distinguished from what particular individuals or cultural groups *say* is good or bad.

cause there probably is no such thing as a self-evident truth in Euclid's sense. The primary principles of logic and mathematics are believed to be those rules of reasoning (symbol manipulation) which have been found by trial to mediate valid (i.e., practically dependable or adaptive) conclusions. The formulation of these principles has taken centuries and is, by no means, complete even now. Thus, scientific theory requires for the derivation of valid theorems (1) sound scientific principles, and (2) sound logical rules for the mediation of the deductive process. Therefore, each empirically verified scientific theorem tends to validate both the scientific principles employed in its derivation and the logical rules whereby the scientific principles were transformed into the theorem. Thus, logical rules are validated in the same way, and indeed, at the same time, as are scientific principles. Accordingly, we conclude that the innate constitution of the "mind" also fails to yield a dependable basis for the validation of ultimate ethical principles.

SECTION OF ANTHROPOLOGY

JANUARY 22, 1945

DOCTOR WENDELL C. BENNETT, Associate Professor of Anthropology, Yale University, New Haven, Conn.: *Interpretations of Andean Archeology.*

In the Andean region of South America, archeological techniques and methods have been largely directed towards field recording and chronology. The aim of field recording is to preserve an excavation record for later analysis and interpretation. The ideal record would be one which allowed laboratory reconstruction of the plan of a grave, a temple, or a refuse site. The techniques employed for this are obviously not adaptable to broad cultural generalizations.

The emphasis on chronology has likewise led to the utilization of specialized forms of evidence. In fact, many of the archeological monographs omit such standard topics as grave type, architecture, weaving and metallurgy. For example, there was, to my knowledge, no published statement that the Nazca Period lacked significant architecture until 1944. This is not a criticism, since chronology is a basic need in Andean archeology. However, it is important to note clearly the unit being studied since chronology can be both cultural and regional. That is, an established chronology may represent a cultural continuum with change over time, or the successive periods of occupation of a specific region. It seems obvious that the potentialities of archeological studies of acculturation depend on having a cultural chronology.

Most Andean chronologies are regional in that they refer to sequences in sites, valleys, or departments. Further analysis and interpretation depends on the grouping of local sequences into larger regional units within which the component cultures and periods are interrelated. In other words, the concept of culture area is introduced, particularly that of Kroeber's "intensive" area, which represents "a substantial unit of historical development, or of a prevailing characteristic current of culture."

For example, the Central Andean region, that is, Coastal and Highland Peru and the altiplano of Bolivia, seems to meet the tests of an intensive culture area. At an early date, a basic pattern was es-

tablished which not only united the component cultures, but also persisted with sufficient strength to absorb and remold outside influences. This can be demonstrated in a number of ways. The intensive agricultural subsistence is a constant. Such domesticated plants as potatoes, corn, quinoa, manioc, beans, peanuts, and many fruits, are common to all of the cultures in spite of the fact that all the plants do not grow in any one local region. The domesticated llamas, alpacas, and guinea pigs are found everywhere, and the cultivation techniques always include the digging stick, irrigation, terracing, fertilizer, and crop rotation. The types of clothing and shelter show no great variation, and the arts and crafts show about the same development, even including some specific design elements, such as the ray fish, the feline, and the trophy head. In all periods, population concentration, class distinctions, religious organization, and political units above the local group, are implied.

In terms of the culture area concept, the greatest inconsistency in the unity of the Central Andes might seem to be the environment. All geographers emphasize the contrast of the desert coast and the high mountain plateaus and valleys. However, in terms of the common culture pattern outlined above, the environmental contrasts are not too great. In both the Highland and Coastal valleys, there are large areas suitable to digging stick cultivation; that is, areas without extensive forests, swamps, deep rooted grasses, and leached out soils. Although the topography is varied, there are no insurmountable barriers. In terms of travel by foot, a sand desert is about as difficult as a mountain trail. In fact, in cultural-geographical terms, there is a far greater contrast between the Central Andes, as a whole, and the grass covered Argentine pampas, the Amazonian tropical forest, the Chilean rain forest, and even the Ecuadorian paramos which are but little suitable to herding llamas and alpacas.

Granting that the Central Andes represents a valid intensive culture area, a relative chronology for the total region can be established. At the present time, there appear to be three generally pan-Central Andean horizons with intermediate local developments. In simplified form, these consist of an early Chavín horizon, a middle Tiahuanaco horizon, and a late Inca horizon. Between the Chavín and Tiahuanaco horizons are local "Early" Periods, such as Mochica on the North Coast, Nazca on the South Coast, and Recuay in the North Highlands. Between the Tiahuanaco and Inca horizons are local "Late" Periods, such

as Chimu on the North Coast, Ica on the South Coast and Decadent Tiahuanaco in the South Highlands. The Spanish Conquest marks the upper time limit of the Inca horizon.

An intensive culture area with its relative chronology can be subjected to various types of analysis. One may start with the contemporary Indians of Peru and Bolivia and trace aspects of their culture back through the historical accounts of the Inca into the archeological past. In such a way, new significance is given to much of the archeological interpretation. Likewise, trends of development within the area can be examined. It can be seen that art styles change from realistic in the Early Periods, through stylization in the Middle Periods, to geometric in the Late and Inca Periods. Habitation patterns can be seen to change from small villages to extended cities, implying at the same time a marked increase in population. Technological trends can be examined. For example, the weaving shows no significant technological advancement, but rather changes due to local preferences for designs and techniques, while the metallurgy shows a developmental technological sequence.

Studies of the culture areas of North America have shown that the component groups may still differ markedly from each other. This seems equally true in the Central Andes, even within the same relative time horizon. Such differences do not seem to be due to lack of contact with other groups in the area, but rather to distinct patterns of cultural orientation, particularly reflected by the leisure time activities of the population. In the Central Andes, leisure time above the economic requirements of food, shelter, clothing, and protection was undoubtedly available for some or for all of the population. Archeologists cannot analyze all patterns of utilization of such leisure time, but some suggestions are possible through examination of evidence of wealth concepts, sharp class differences, religious enrichment, public work projects, and the like. Two periods are discussed (and illustrated) as samples: the Nazca-Paracas on the South Coast, and the Mochica on the North Coast. Both are roughly contemporaneous and the coastal environment is about the same.

The Nazca and the Necropolis Period of Paracas are closely related and are treated here as a unit. As such, the materials are found in the Nazca, Ica and Pisco Valleys, all of which are limited in cultivable soil, so that the absolute size of the population was never great. There is no evidence of large scale buildings, pyramids, or

public work projects which would indicate organized mass labor. Metallurgy is limited to hammered gold objects, but ceramics are well developed, and decorated with polychrome designs, representing ornate and elaborated religious figures.

Textile weaving for burial purposes is the outstanding development. Large mummy bundles are found which contain innumerable pieces of finished cloth. Such pieces show none of the usual signs of wear from use in daily life, and many are made for the size of the bundle rather than for the size of the living. Weaving requires a great amount of time, when all factors are considered, such as trade for raw materials, spinning, dyeing, the actual loom weaving, and embroidery finish. The technical standards are high and still it is evident that large numbers of weavers were able to meet these standards. One is impressed by the number of man hours of labor directed, not towards the living, but rather towards the glorification of the dead.

In brief, there is little archeological evidence for marked class distinctions, organized labor patterns, highly formalized religious or political institutions. The Nazca-Paracas pattern might be described as a grave-oriented use of leisure time.

The Mochica Period is centered in the Viru, Moche, and Chicama Valleys, but spreads beyond these in two directions. Many large pyramids and other public works suggest organized mass labor. Weaving is little developed, or at least it has not been preserved, although ceramic designs indicate weaving under supervision. The grave contents are largely ceramics, the manufacture of which does not represent the same time factor as does the hand weaving of textiles.

The ceramic designs suggest rather marked class distinctions. These are demonstrated by scenes of swimmers towing a man on a raft, honored guests being served by menials, and individuals carried in litters. There is evidence of many specialized groups, such as runners, priests, and warriors. The ceramic designs also indicate an interest in worldly things by faithful portrayal of plants, animals, scenes of hunting, fishing, and warfare, and diseases and punishments. In general, the Mochica pattern suggests that leisure time led to class distinctions and corvée labor.

Archeological observations on these two patterns can be made historically. The Tiahuanaco style, which marked the Middle Period time horizon, mixed readily with the Nazca. The potters made vessels, with new shapes and designs. The weavers turned to ambitious tapes-

tries, but still designated for the grave. Tiahuanaco style did not mix with Mochica, but rather replaced it, or, more likely, drove it temporarily to the north. Furthermore, the Nazca pattern did not expand geographically but remained limited to three valleys. The Mochica, on the other hand, spread over seven valleys, and its influence is noted in the North Highlands, and on the Central Coast.

The Nazca pattern disappeared with the Tiahuanaco, never to be reformulated. The Ica Late Period on the South Coast has been called "Late Nazca" only because of the valley in which it is found, but its ceramics, buildings, and weaving show practically no influences from the earlier pattern. The Mochica pattern, however, persisted, in spite of local interruption by Tiahuanaco. The Chimu Late Period is an extension of the earlier Mochica. The question of this relationship has never been raised; the problem, rather, is to express it.

It is interesting to speculate that the persistence of Mochica may have been due to the corvée labor patterns. Ruth Benedict has suggested that, wherever the American Indians have survived in numbers up to the present time, the old political patterns were based on corvée labor. The Mochica pattern seems a case in point.

SECTION OF PHYSICS AND CHEMISTRY

JANUARY 26 AND 27, 1945

Conference on *"Surface Active Agents."*

The Section of Physics and Chemistry held a Conference on "Surface Active Agents," as the third in the series for the Academic Year 1944–1945. Doctor M. L. Anson, Continental Foods, Inc., Hoboken, New Jersey, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by M. L. Anson.

"The Structure and Properties of Solutions of Colloidal Electrolytes," by A. W. Ralston, Armour Laboratories, Chicago, Illinois.

"Surface Active Agents at Interfaces," by Earl K. Fischer and David M. Gans, Interchemical Corporation Research Laboratories, New York, N. Y.

"Certain Aspects of the Chemistry of Surface Active Agents," by Donald Price, Interchemical Company, New York, N. Y.

"Properties Involving Surface Activity of Solutions of Paraffin Chain Salts," by Leo Shedlovsky, Colgate-Palmolive-Peet Company, Jersey City, New Jersey.

"Surface Active Agents in Biology and Medicine," by E. I. Valko, Onyx Oil and Chemical Company, Jersey City, New Jersey.

"The Nature of the Bacteriacidal Action of Surface Active Agents," by Rollin D. Hotchkiss, Rockefeller Institute for Medical Research, New York, N. Y.

"Surface Active Agents in Industry," by R. R. Ackley, Mellon Institute, Pittsburgh, Pennsylvania.

"Surface Active Agents in Ore Flotation," by M. D. Hassialis, School of Mines, Columbia University, New York, N. Y.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1945

Two prizes of $200. each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Dinner, December, 1945, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

CONDITIONS:

(1) Eligibility. Authors and coauthors shall be members in good standing of The New York Academy of Sciences or one of the Affiliated Societies, prior to submission of the manuscript.

(2) Date. Papers are to be submitted on or prior to **October 1, 1945**, to the Executive Secretary of The New York Academy of Sciences, at The American Museum of Natural History, Central Park West at 79th Street, New York, N. Y.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1945 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal pub-
icity initiated by the author regarding the prize paper. If published
lsewhere, six copies of each prize paper must be deposited shortly after
ublication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,

Central Park West at 79th Street, New York, N. Y.

EUNICE THOMAS MINER,
Executive Secretary.

NEW MEMBERS

Elected January 25, 1945

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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 7 MARCH, 1945 No. 5

SECTION OF GEOLOGY AND MINERALOGY[1]

FEBRUARY 5, 1945

DOCTOR JOHN G. BROUGHTON, Assistant State Geologist, New York State Museum, Albany, N. Y.: *Geology of Ceramic Materials in New York State.*

Certain non-metallic minerals occurring in New York are of potentially great importance in the ceramic field. The particular physical properties or firing behavior of talc, tremolite, wollastonite and diopside fit them for specialized ceramic applications. Experimental work in this field has been carried on by C. R. Amberg at the New York State College of Ceramics.

It has been shown that talc from St. Lawrence County, New York, is satisfactory for ceramic use after some form of beneficiation such as flotation or electro-static separation. This is necessary because of the high lime content resulting from intimate mixture of talc with unaltered tremolite, the parent mineral. It has been thought that the tremolite was developed by contact metamorphism of quartz-rich pre-Cambrian dolomite. Recent work by James Gilluly has resulted in a new interpretation of the formation of tremolite. His well-documented field study indicates that silication by solutions of igneous origin took place along shear zones in the dolomite. This concept of the control as structural rather than stratigraphic should have an important effect on future development of the district.

[1] No meeting of the Section of Biology was held in February.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 7, No. 5, March, 1945.

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Editor: Roy Waldo Miner.

Executive Secretary: Eunice Thomas Miner.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

Contact metamorphic rocks containing large bodies of wollastonite and canaanite diopside are found near Willsboro, Essex County, on the eastern slope of the Adirondacks. These minerals occur in bands of Grenville limestones and skarns which have been caught up in gabbroic anorthosite. The wollastonite is associated with andradite garnet and salite. This mineral assemblage has apparently developed from contact metamorphism of siliceous and magnesian limestones. There is also evidence of a later low-temperature hydrothermal stage with introduction of iron-rich solutions and re-crystallization of part of the wollastonite.

The canaanite diopside is accompanied by quartz and graphite.

Firing tests of wollastonite and of diopside mixed with ZrO_2 have developed interesting tile bodies, some of which are extremely light in color and have a hardness in Moh's scale of 8.

SECTION OF PSYCHOLOGY

February 19, 1945

Doctor Karen Horney, New School for Social Research, New York, N. Y.: *The Role of Conflicts in Neuroses, or Modern Psychoanalysis.*

No abstract of this paper has been received.

SECTION OF ANTHROPOLOGY

FEBRUARY 26, 1945

DOCTOR GEORGE DEVEREUX, Department of Sociology, Wellesley College, Wellesley, Mass.: *The Logical Foundations of Culture and Personality Studies.*

It is frequently advisable to begin the discussion of the logical foundations of a science with a definition of the author's own philosophical position. This procedure enables one to avoid numerous misunderstandings. It is, furthermore, a highly economical procedure. Definitions can be stripped of a lot of verbiage whose sole purpose is the avoidance of types of ambiguousness resulting from a lack of understanding of the author's position with regard to some of the fundamental concepts and problems of philosophy, e.g., reality vs. subjectivity. My own position may be best defined as "Poincaréan" conventionalism.[1]

(1) I believe that the two statements: (a) "The external world exists," and (b) "It is more convenient to assume that the external world exists" are equivalent propositions.

(2) It is possible to describe given and possible aspects of a phenomenon, without raising the issue of the subjectivity or independent reality of that phenomenon.

(3) Two sets of postulates from which the same conclusions can be drawn are equivalent.

(4) If a phenomenon admits of one explanation, it will also admit of any number of other explanations, all equally satisfactory.[2] The term "explanation" has been adequately defined by Meyerson.[3] An explanation is a process whereby a given phenomenon is reduced to other phenomena. All explanations are partial ones, since the complete explanation of a phenomenon implies denying the phenomenon to be explained, by reducing it entirely to other, more "basic" phenomena.

In fact, I shall nowhere concern myself with the problem of the "existence" of anything, except that I shall attempt to avoid the culturalist fallacy, which is obviously untenable.

All culture and personality studies appear to rest upon the basic assumption that culture influences personality. This assumption

[1] **Poincaré, H.** The foundations of Science. New York. 1913.
[2] **Poincaré, H.** Electricité et Optique. Paris. 1901.
[3] **Meyerson, E.** De l'Explication dans les Sciences. Paris. 1921.

distinctly implies that there "exists" a *sui generis* phenomenon, A ("culture"), which acts upon another *sui generis* phenomenon, B ("personality").

It is my thesis that neither this assumption, nor its obvious implications are methodologically necessary in the pursuit of culture and personality studies. I have offered elsewhere[4] a definition of the methodology of culture and personality studies which does not rest upon this assumption: "If, at any stage of the individual's life, we correlate our findings concerning his individual configuration *at* that time with the structure of the field wherein the individual has moved *up to* the time to which our formulation of his personality refers, we have made a significant statement concerning the interrelation of culture and personality." Although this statement is clear enough as it stands, I have taken further precautions against any attempt to read platonic idealism into the above statement, by previously defining culture as a highly structured and patterned field within which the individual has a certain mobility.[5]

In fact, culture and personality studies are, in a sense, merely a subdivision of general studies in conditioning or learning. If, in the following pages, I discuss suitable definitions of culture and of personality, I do so defensively, because every time the conception of culture as a phenomenon *sui generis* is ejected by the front door, it is invariably smuggled in once more by the back door, in a more or less transparent disguise.

The pitfalls of the "culturalist fallacy" in culture and personality studies have been clearly formulated by Hallowell.[6]

"It is hard to see how culture—an abstract summation of the mode of life of a people—can exert an influence except as it is a definable constituent of the activities of human individuals in interaction with each other. In the last analysis, it is individuals who respond to and influence one another. Culture as Bidney[7] has pointed out "is not an efficient cause and does not develop itself, hence it is not capable of interacting with any other entity." To argue otherwise leads to what he calls the "culturalist fallacy," which is based on the assumption

[4] **Devereux, G.** Mohave Culture and Personality. Character and Personality. 8: 91–109. 1939.

[5] **Devereux, G.** Ibid.

[6] **Hallowell, A. I.** Sociopsychological Aspects of Acculturation (in) **Linton, R.** (Ed.) The Science of Man in the World Crisis: 171–200. New York. 1945.

[7] **Bidney, D.** On the Concept of Culture and Some Cultural Fallacies. American Anthropologist (n.s.) **46**: 30–44. 1944.

that "culture is a force that may make and develop itself and that individuals are but its passive vehicles or instruments."

I feel that this statement is correct in every respect, and formulates with great precision the basic problem of culture and personality studies. It may be suggested, *en passant,* that the difficulties arising from the correctness of the Hallowell-Bidney statement may—at least unconsciously—be at the root of all attempts to consider culture as a phenomenon *sui generis,* having independent reality.

Three erroneous reactions to the Hallowell-Bidney statement are possible:

(1) It makes culture and personality studies entirely impossible. The concrete accomplishments of students of culture and personality, including those of Hallowell himself, make such an interpretation preposterous.

(2) The Hallowell-Bidney statement abolishes the autonomy of the field of culture and personality studies and transforms this discipline into a branch of social psychology. Such an interpretation is probably incorrect on the one hand, and, on the other, makes no particular difference.

(3) The meaning of this statement can be distorted in such a manner as to make it appear to be an instance of what F. Kluckhohn[8] calls the psychological fallacy, and which I prefer to call the psychologistic fallacy (i.e., the inability to see the forest for the trees). Such an interpretation is obviously erroneous, both in terms of the Hallowell-Bidney statement, and in terms of F. Kluckhohn's definition of the psychological fallacy.

The way out of this predicament is already indicated in Hallowell's own formulation of the problem. I refer to the term "abstract summation" and to his reference to a "definable constituent of the activities of human individuals in interaction with each other."

Let us consider, first, the concept "abstract summation." It can be subdivided into two parts, each part being an abstract summation in its own rights:

(1) It is possible to offer an abstract summation of the social and cultural norms of a suitably selected group of persons.

[8] Kluckhohn, F. A Consideration of Method in the Social Sciences. (Mimeographed, n.d.).

(2) It is likewise possible to offer an abstract summation of the prevalent concrete modes of behavior of a suitably selected group of persons.

It is usually possible to perform a further abstract summation of the above mentioned two special summations.

The principle underlying this distinction is partly implicit in Linton's[9] distinction between overt and covert culture, in C. Kluckhohn and Kelly's[10] distinction between implicit and explicit culture, as well as in Bidney's[11] distinction between theory and practice.

The above distinction between two types of abstract summations was made in terms of the subject-matter of the summations.

Another useful distinction may be made in terms of the type of person who performs the act of summation:

(1) Human beings can, and, in fact, do perform such abstract summations of the norms (i.e., "This is our custom") and of the practices (i.e., "People tend to act in this manner") characteristic of their own group.

(2) Human beings, in general, and social scientists, in particular, can, and, in fact, do perform such abstract summations of the norms (i.e., "Mohave Indian descent is patrilineal") and of the practices (i.e., "The Mohave family is highly unstable") characteristic of, or arbitrarily imputed to the outgroup.

The first of the two types of summations just mentioned is presumably the one to which Hallowell refers in speaking of the definable constituents of human behavior. Human beings do develop a conception of their own culture. Nor is that all. They also reify this conception, and respond to this "collective representation" (to be defined in Durkheimian terms) in a highly distinctive manner, i.e., when the Hopi characterize socially inacceptable or deviant behavior as "ka-Hopi" (un-Hopi).[12]

This means, in fact, that ordinary human beings do respond differently to trees and to a forest. It suffices to think, for example, of the differential effects of referring, in a poem, either to trees to a forest.

[9] **Linton, R.** The Study of Man. New York. 1936.

[10] **Kluckhohn, C. & W. H. Kelly.** The Concept of Culture (in) **Linton, R.** (Ed.) The Science of Man in the World Crisis: 78–106. New York. 1945.

[11] **Bidney, D.** (*Op. cit.*) See also **Bidney, D.** On Theory and Practice. University of Toronto Quarterly **7**: 113–125. 1937.

[12] **Simmons, L.** (Ed.) Sun Chief. New Haven, 1942.

The psychological process of reification, and of differential response to such a reification, is by no means unique. It resembles the process whereby the child transforms his father into a superego. One must take it for granted, of course, that the father has a superego of his own, which determines the kind of father (i.e., "typically Mohave" or "atypically Mohave") he makes. In this sense, the father is but an agent of society, or, to be more specific, of his particular abstract summation of his own socio-cultural setting. I have, in fact, shown elsewhere[13] that it is sometimes more profitable to emphasize the "transformation" of society into the father, rather than to stress the "transformation" of the father into the superego. Since no chain of arguments of this type is profitably prolonged beyond a certain point, technical expediency alone will decide which of these two sequences one emphasizes in a given context.

Here, I think, we are on sound psychological grounds, and Hallowell's thesis may be considered as proven.

I cannot sufficiently stress the fact that an abstract summation of this type is present in the makeup of every individual, regardless of whether it was handed to him ready-made by his "mediators of culture," or whether he has developed it independently. Likewise, it is of no importance, whatsoever, in this context whether or not he realizes that he (or some mediator of culture) has evolved this abstract summation on the basis of a study of concrete individuals, either in his own lifetime or on some past occasion.

I suggest, therefore, that it is not even necessary for the student of culture and personality to reify culture. The members of the society which he studies have already performed this task for him. It is, perhaps, this reified abstraction which may be thought of as *that* "culture" which influences the behavior and "personality" of the individuals composing that group, both directly, as part of any given individual's makeup, and indirectly, through the concrete behavior of other individuals who have likewise "introjected" the same norms and whose behavior our subject refers back to that reified abstraction, precisely in the same manner in which he refers back his own actions to the norms and practices of his group.

Let us now turn to abstract summations made by outsiders and, more specifically, by social scientists. The social scientist has an ad-

[13] **Devereux, G.** Social Negativism and Criminal Psychopathology. Journal of Criminal Psychopathology 1: 323–338. 1940.

vantage over the student of gases. Gas molecules do not enunciate the laws of probability, nor the kinetic theory of gases, nor the laws of statistical mechanics, whereas the members of a given society, which the social scientist studies, do precisely that, whenever they perform the operation known as "abstract summation," and enunciate it in the form, "This is our custom," or, "our women tend to be flighty."

Sometimes, however, the student of culture is called upon to perform a further operation of his own. He may find it expedient to evolve a special type of abstract summation in connection with a given culture. Such an abstract summation would be a conceptual scheme establishing a functional nexus between the norms and actual practices of a given group, just as the student of personality may have to evolve a conceptual scheme to establish a functional nexus between what the individual says he ought to do (or thinks he does) and what he does in fact.

It is probable that there is always a need for such a conceptual scheme or abstract summation. In certain instances, it is, however, especially necessary. This is the case, for example, whenever theory and practice diverge very sharply, or whenever the individual has to go to great lengths, rather frequently, to rationalize his actual behavior. It is also necessary whenever, due to various causes, mostly historical, the norms of a given group are poorly integrated and full of contradictions and gaps.

Last, but not least, it is necessary to develop such a conceptual scheme or abstract summation whenever norms and practices vary extensively between the several segments of a complex society.

Societies, whose study calls for further summations of this type, are, with the exception of the polysegmented ones, generally more or less disorganized or in a state of flux. The technical difficulties of evolving appropriate conceptual schemes or abstract summations are usually considerable in these instances, but do not, as a rule, involve fundamental problems of a logical order.

The situation is different, however, with regard to polysegmented societies. Polysegmentation—especially in the form which Durkheim[14] calls "organic"—presents complex problems of its own, which—while present in simple form in every single society differentiating between

[14] **Durkheim, E.** De la Division du Travail Social (2nd ed.) Paris. 1902; **Durkheim, E.** The Rules of Sociological Method. Catlin, G. E. G. (Ed.); **Solovay, S. A. & J. H. Mueller,** (Transl.) Chicago. 1938.

people on any basis whatsoever, i.e., in terms of age, sex, status, etc.—raise certain questions regarding the exact limits of the usefulness of the *specific* "basic personality" conceptual scheme developed by Kardiner and Linton[15] though they appear to raise no questions regarding the validity of the special technique pertaining thereto.

In fact, the problems raised by polysegmentation are so complex that they cannot be tackled without first defining "personality."[16]

Definitions of personality are very numerous, one of the best known being that of Mark A. May,[17] whose great usefulness for the student of culture and personality has been shown by Kluckhohn and Mowrer.[18] Like most really useful definitions, it is a simple one: "Personality is the social stimulus value of the individual." This definition has several advantages. It avoids any reification of personality. It is inductive. It implies that different observers will be differently stimulated by the subject, and, hence, will develop different conceptions of his personality. We take our cue from this definition and proceed accordingly.

If we observe a given subject over a certain period of time, we automatically come to think of him in a certain way and to expect certain actions from him. Rigorously speaking, we integrate his behavior in a distinctive manner by performing two operations:

(1) We establish a functional nexus between his several modes of behavior, by seeking for a unique conceptual scheme referring to that individual, to be labelled "personality." This conceptual scheme accounts—in theory at least—for all peculiarities of behavior displayed by the subject. Roughly speaking, we seek to "explain" that individual's behavior in a unified manner.

(2) We test the validity of this conceptual scheme by predicting the future behavior of that individual; i.e., we examine our conceptual scheme "personality" and, by logical deduction, attempt to discover

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[15] **Kardiner, A.** The Individual and His Society. (With a foreword and two ethnological reports by **Ralph Linton**). New York. 1939.

[16] I have completely disregarded, in this context, the hypothesis that culture is an emergent. I have shown elsewhere (**Devereux, G.** A Conceptual Scheme of Society. American Journal of Sociology **45:** 687–706. 1940) that it is not *necessary* to consider society as an emergent. Precisely the same logic can prove that we do not *need* to think of culture as an emergent. I hold that once the absence of any need for a hypothesis is proven, it is legitimate in science to lose all interest in that hypothesis.

[17] **May, M. A.** A Comprehensive Plan for Measuring Personality. Proceedings and Papers of the Ninth International Congress of Psychology: 298–300. Princeton. 1930.

[18] **Kluckhohn, C. & O. H. Mowrer.** Culture and Personality: A Conceptual Scheme. American Anthropologist (n.s.) **46:** 1–29. 1944.

the further properties of the system "individual X" not yet disclosed by actual observation.

Summing up, we view personality as a conceptual scheme, i.e., as a unified "explanation" and as a means of prediction. This definition suffices for our purposes. Nothing would be changed, as far as results are concerned, if we took a position regarding the independent reality or subjectivity of "personality." It is implicit, of course, in our philosophical position, that one may formulate several conceptual schemes accounting equally well for the occurrence of observed behavior and predicting equally accurately future behavior. None of these "explanations" and/or predictions is complete, however, nor is it necessary to assume that they overlap fully, either as regards the areas of behavior to which they refer, or as regards the concepts constituting the several schemes in question. It is important to bear this in mind.

I am now prepared to correct a deliberate misstatement which I made above for the sake of simplicity.

I have spoken above of the conceptual scheme "personality" as an "explanation." In fact, a conceptual scheme is not, and cannot be, an explanation in the sense in which Meyerson uses this word. A conceptual scheme is not a phenomenon to which another phenomenon is reduced, nor is it such a process of reduction. However, it can serve as a frame of reference within which explanations can be made. Hence, it is, loosely speaking, a means of explanation, but not an explanation *per se*. This distinction is of some importance.

Another grave logical error is the assumption that personality can be explained or reduced to other phenomena, which may be viewed as factors or determinants of personality, which we defined as a conceptual scheme herein above. It is impossible to explain a conceptual scheme in Meyerson's sense. The sources of this whole confusion are very simple. They are part of the recurrent failure to distinguish between a class and the members of that class. Even a class having only one member is not the same thing as that member.[19]

Briefly expressed, only a phenomenon, or a set of phenomena, can be explained in Meyerson's sense. A conceptual scheme can merely be analyzed. Concretely, behavior can be explained. Personality can only be analyzed.

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[19] **Russell, B.** Principles of Mathematics. Vol. I. Cambridge. 1903.

Let us attempt to approach this problem in a practical manner, in order to avoid the expository complexities of the subject.

Let us now distinguish between two types of concepts. There are, first of all, the inductive generalizations, exemplified in the concept "the solids." Next, there are the analytic variables, exemplified in the concept "solid" or "solidity." Epistemologically, the concept of solid bodies is prior to the concept of solidity. The latter was developed from the study of solid bodies.

Somewhat anticipating the course of our analysis, explanations of behavior tend to be formulated in terms of inductive generalizations, whereas the conceptual scheme "personality" tends to be constructed out of analytic variables. It should be noted that this appears to be a rule of thumb statement rather than a dogmatic one (I use the word "dogmatic" in the philosophical sense).

Let us now inquire into the genealogy of these concepts.

Inductive generalizations (and, hence, also analytic variables) result from the study of sets of phenomena, studied *both jointly and severally*. This last specification refers to Russell's[20] distinction between "all" and "every."

We naturally assume that no set actually studied is complete, but we do assume that it is a representative sample of the complete set (i.e., no human biologist ever studied all men who ever lived).

Now, it is of crucial importance to realize that two such sample sets may have the same members, i.e., that their membership overlaps more or less completely. This does not mean, however, even where the membership of both classes is rigorously the same, or when the larger of the two memberships comprises all members of the smaller aggregate, that the two sets or classes are logically identical (i.e., all members of a sample group of Mohave Indians may also be members of a sample set of *Homo sapiens*. Yet "Mohave" and "*Homo sapiens*" are logically distinct types).

For the sake of simplicity, let us consider only this concrete instance. It is possible to evolve a set of propositions which will refer only to the Mohave Indians under study (i.e., we can write a Mohave ethnography). Likewise, it is possible to evolve a set of propositions which will apply specifically to the other sample group, e.g., the one composed of *Homo sapiens* (i.e. we can write a book on human biology).

[20] **Russell, B.** *Op. cit.*

Now, broadly speaking, Mohave ethnography is a conceptual scheme, and human biology is a conceptual scheme.

Two things can be done at this juncture with regard to concrete individuals.

(1) We can refer the behavior of a given individual (temporal ensemble) to the behavior of a set of individuals (i.e., Mohave or *Homo sapiens*). Rigorously speaking, we refer it to certain inductive generalizations. These inductive generalizations may be considered as factors or as determinants, and were derived from the study of a spatial ensemble. They can be used as "explanations" of the concrete behavior of our subject, in the strictly "Meyersonian" sense of the word. Naturally, it is assumed that one shall avoid the logical fallacies discussed by Russell[21] in connection with such statements as "*A* is a typical Frenchman" and "Napoleon had all the qualities that make a great general." This type of explanation is what Kluckhohn and Mowrer[22] appear to have had in mind.

(2) I can also analyze the conceptual scheme, "personality," or, more specifically, "personality of X," by tracing the genealogy of the concepts constituting that conceptual scheme. This does not constitute an "explanation," in Meyerson's sense. It does, however, provide us—loosely speaking— with a frame of reference for the explanation of behavior—*but not of personality* (obviously, this does not mean that we are now considering "personality" as a phenomenon *sui generis*. We do not assert that a conceptual scheme exists).

The distinction between the explanation of behavior and the analysis of the conceptual scheme "personality" is logically important. It is very interesting to note that both types of approach tend to yield the same results in terms of the understanding and prediction of the concrete behavior of a human being.

If that be the case, those impatient of logical discussions may inquire why so much space has been devoted to the abstract problem of the analysis of the conceptual scheme "personality." In justification of this procedure, it may be stated that the problem, thus formulated, at once removes the *substance* of debates of the nature vs. nurture type and reduces them to purely formal distinctions. The question is no

[21] **Russell, B.** Introduction to Mathematical Philosophy. London 1919. Cf. also **Whitehead, A. N. & B. Russell.** Principia Mathematica I. (2nd ed.) Cambridge. 1925.

[22] **Kluckhohn, C. & O. H. Mowrer.** *Op. cit.*

longer "Does biology or society determine human behavior?", but "Are biological or sociological concepts being used in a given context?"

Let us assume that we have developed some unique and reasonably complete conceptual scheme "personality." It is made up of biological, sociological, etc., concepts, most of them analytic variables. This is the ideal case. One cannot get very excited about a numerical tabulation of the biological, sociological, etc., concepts involved.

It is probable, however, that this ideal has not yet been reached and that we are using not one, but several conceptual schemes of personality. One such conceptual scheme may be made up of biological concepts, another one of sociological concepts. The question is no longer "Where does human nature end and nurture begin?," but "Where is it more economical to switch from one conceptual scheme or frame of reference to another conceptual scheme or frame of reference?" This, I submit, is a purely technical question, and expediency alone suffices to decide when to desist from hairsplitting and to switch over to another frame of reference, instead.

It suffices to mention such technical issues in a logical analysis, but it is not necessary to expatiate on them.

There exists, however, another problem of quite a different type, which also arises from the fact that the membership of certain samples tends to overlap either partially or completely. It is a problem connected with the theory of the "basic personality," a conceptual scheme developed rigorously and used carefully by its originators, but so abused by some of those who took it over ready-made that it is sometimes actually used as an "explanation"(!) of the concrete behavior of individuals.

Logically, the problem is so complex that it seems more expedient to deal with it in terms of concrete examples.

Can the personality of John Doe, a native of New York, be more completely understood by assigning him to the American complex, in general, or by stressing his urban habitat and treating him, therefore, primarily as a city dweller on a par with the inhabitants of Paris and Calcutta? (One should consider the fact that the very possibility of there being large cities is part of the American complex, and that New York differs in many respects from Paris and Calcutta, precisely because it is an American city.)

Shall we explain John Doe's behavior in terms of the prevailing American norms and practices, or in terms of the prevailing urban norms and practices? Shall the conceptual scheme "personality of John Doe" contain principally sociological or "urbanistic" concepts? Shall we treat the sociological frame of reference on a par with the urbanistic frame of reference? In brief, is John Doe a typical American or a typical urban man? Is his "basic personality" American or urban?

This problem cannot be solved in rigorously logical terms without starting with a discussion of the meaning of "basic personality." Unfortunately, we are immediately in the midst of Russell's aforementioned discussion regarding statements of the type "*A* is a typical Frenchman" and "Napoleon had all the qualities of a great general." This, in turn, leads us to the problem of mathematical types, which it is almost impossible to explain in non-technical language.

Perhaps the nearest I can come to a simple explanation of this problem, as it refers to our topic, would be to quote directly from Russell[23]: "How shall I define a 'typical Frenchman'? We may define him as one 'possessing all qualities that are possessed by most Frenchmen.' But unless we confine 'all qualities' to such as do not involve a reference to any *totality* [my italics] of qualities, we shall have to observe that most Frenchmen are *not* typical in the above sense, and therefore the definition shows that to be not typical is essential to a typical Frenchman. This is not a logical contradiction, since there is no reason why there should be any typical Frenchman; but it illustrates the need of separating off qualities that involve reference to a totality of qualities from those that do not." Again, with reference to Napoleon and his qualities, Russell observes "I must define 'qualities' in such a way that it will not include what I am now saying, *i.e.*, 'having all the qualities that make a great general" must not be itself a quality in the sense supposed."

I now assert something that is completely obvious in terms of Russell's theory of types (just quoted in the form of a concrete example of interest to the student of culture and personality). It is entirely illegitimate to consider "basic personality" as a quality logically on a par with such other quality or qualities as constitute "basic personality" or exist outside of it, as "idiosyncratic" qualities, for example.

[23] See footnote 21, Introduction, etc.: 189.

Indeed, basic personality is a totality of qualities. This is crucial for our whole approach. When we say that John Doe is a typical American or urban man, we assert—if we disregard this warning—that he possesses all the qualities possessed by most Americans or by most urban men, including the quality (and therein lies the error) of having the totality (i.e., basic personality) of the qualities in question. This, of course, is a vicious circle.

Kardiner and Linton seem to have avoided completely this misinterpretation and misuse of the conceptual scheme "basic personality." Unfortunately, the same cannot be said of some others who have used this conceptual scheme in a rough and ready manner.

(It should be noted that the "Napoleon" vicious circle is likewise avoided in Linton's concept of the status personality.[24])

The purpose of the above discussion was to clarify an aspect of the meaning of "basic personality" not specifically elaborated by Kardiner and Linton, and to define clearly the manner in which this conceptual scheme is used in the following discussion.

It is my belief that the conceptual scheme "basic personality" (and the special technique connected therewith) is a far more powerful tool than has been suspected hitherto. There are, in fact, several basic personalities, all cut logically on the same pattern, although not cut on the same pattern psychologically, i.e. as regards content. The original concept, as such, is well known. The Tanala resemble each other in a distinctive way, which differs from the equally distinctive way in which the Marquesans resemble each other. Hence, Kardiner and Linton speak of a basic Tanala personality and of a basic Marquesan personality. This is, indeed, *a* basic personality, though not necessarily the only one, and perhaps not even the most basic one, depending on the way in which the term "basic" is used in our discourse.

In fact, one may be typically American, typically urban, typically middle-class, etc., simultaneously, strictly in Russell's sense of the, correctly used, word "typical."

In fact, one is typically human, before one is anything else. I am indebted to Dr. Géza Róheim for the suggestion that the most basic of all basic personalities is the one connected with the fact that we are all human. This is the Kluckhohn-Mowrer category "universal," except in one respect. These authors rightly stress that one's self-ascribed and

[24] **Linton, R.** The Study of Man. *Op. cit.*

socially recognized status as a human being is part of the complex of being "typically human." I cannot, however, accept their assertion that one may legitimately exclude from this category, as it is used in culture and personality studies, such deviants as idiots. This can be proven empirically. The Romans did not think it necessary to institute Tarpeian rocks for defective animals. The Nazis may loudly deny human status to Jews, and yet they treat them differently, though perhaps worse, than they treat animals. The Hà(rhn)de:a(ng) Moi of Indochina[25] may have devised means whereby a human being can renounce his human status and "become a wild boar," and yet they react differently toward real wild boars and toward nominal wild boars. The "vogelfrei" German outcast of yore, unlike game-birds, was not protected by poaching laws. On the other hand, unless his "vogelfrei" status was known, he had the same social stimulus value as any other human being. Conversely, the Mohave may insist that dogs are persons, and yet they do not treat dogs the way they treat human beings. Nor do students of culture and personality ascribe to Rover the kind of personality which they ascribe to human beings of all kinds.

This "basic personality" is then a conceptual scheme formulated in a distinctive manner and composed of biological concepts and of concepts connected with self-ascribed and socially recognized human status. In other terms, the relevant distinctive modes of behavior may be explained most efficiently in terms of biological processes and of human status.

It is possible to deal rather briefly with all other basic personalities: tribal, urban vs. rural, Gemeinschaft vs. Gesellschaft,[26] status, etc. All of these types of basic personalities may be constructed with the help of the Kardiner-Linton technique. The formulation of the problem and the type of understanding desired, as well as the efficiency and the economy of the means employed, will decide whether we choose to emphasize the American, the urban, the leisure class,[27] the occupational status etc. "basic personality" of a given concrete individual.

The fact that the special technique in question, and the conceptual scheme "basic personality" have apparently a far broader application

[25] **Devereux, G.** Functioning Units in Hă(rhn)de:a(ng) Society. Primitive Man **10**: 1–7. 1937.

[26] For a validation of this distinction cf. **Devereux, G.** Human Relations and the Social Structure. A series of lectures delivered before a seminar on "Culture and Personality" in the Department of Anthropology, Columbia University. Winter, 1944.

[27] **Veblen, T.** The Theory of the Leisure Class. New York. 1899.

than was believed originally greatly enhances their importance.

As a matter of fact, in simple societies—such as those from the study of which this conceptual scheme and this technique were evolved—the assorted theoretically distinct basic personalities tend to overlap to a very considerable extent.

The same is not true, however, of polysegmented societies. I have stressed elsewhere that the formulation of the American complex is far more abstract and far less useful in practice than is the formulation of the Tanala complex, for example.[28] The same may be said of the relative abstractness and practical usefulness of such formulations as "the typical American" and the "typical Tanala."[29]

Let us analyze the question from a strictly technical viewpoint.

In polysegmented societies the strictly orthodox type of "basic personality" tends to be very abstract, and, technically, relatively unproductive. This is also true, to a large extent, of societies which are disorganized or in a state of flux, as these terms were defined earlier in this paper. In polysegmented societies, in general, and in disorganized and fluid societies, in particular, the various statuses which, by definition, are necessarily complementary (e.g., husband and wife, employer and employee), are poorly articulated, and, hence, the corresponding modes of behavior (or, conceptually, the corresponding "basic status personalities") tend to have a negligible degree of overlapping. As long as the peasant and the feudal Lord both recognized the God-given nature and necessity of their relationship, and as long as there occurred at least symbolically equivalent "mutual prestations," in a Durkheimian sense, we could perhaps speak with some justification of a *technically useful* mediaeval personality. Today, when the divine rights of manufacturers are not generally recognized by their employees, a similar generalization would be technically less useful. The validity of this statement is best proven by Kardiner's own analysis of the results of a shift in Betsileo economy from dry to wet rice farming.

Hence, in situations of this type, it suffices to postulate, on a purely theoretical level, a "basic personality" in the orthodox sense; and to deal, in practice, principally with other types of basic personalities, e.g., urban, status, etc.

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[28] **Devereux, G.** A Sociological Theory of Schizophrenia. Psychoanalytic Review **26:** 315–342. 1939.

[29] **Devereux, G.** (Review of) **Kardiner, A.** The Individual and His Society. Character and Personality **8:** 253–256. 1940.

It is, however, necessary, both theoretically and practically, to make a distinction between, e.g., American basic personality (urban variety) and urban basic personality (American variety). I am unable to discuss here whether these two formulations belong to one and the same, or to two different logical types. Tentatively, as a target to shoot at, I am inclined to believe that they belong to the same logical type. This appears to be in accord with my initial assumption that there are several kinds (in terms of content, but not in terms of type) of basic personalities. (It should be noted that mere numerical differences in the membership of two classes do not imply differences in degree of abstractness.) Suffice it to say that complex "basic personalities," of the kind just mentioned, appear to be both useful and logically legitimate devices in concrete research work.

We may now turn to the final logical problem of culture and personality studies: The nexus between basic personality and the personality of a concrete individual.

Let us consider, first of all, the general problem of uniqueness, or of the "idiosyncratic." Epistemologically, it is hardly necessary to mention that the concrete individual is prior to any generalization. Yet, sometimes, it is technically expedient to view the individual as the locus in which several abstract classes overlap. In other words, it is technically possible to define the concrete individual as the sole member which several abstract classes have in common. I hold that this recognition, including the enumeration of the classes, is, in fact, an "explanation" of the concrete in terms of the abstract, though it is not, strictly speaking, a Meyersonian type of explanation.

If, now, I succeed in establishing a logical nexus between these various classes, *other than the fact that they share one member in common,* I am actually constructing a conceptual scheme. I now state, as a hypothesis, that a set of classes may overlap in several places, each area of overlapping being confined to a single individual member. I further state, as a hypothesis, that the logical nexus established between these various classes, at the point where they share member A in common, may be distinct from the logical nexus which I may establish between them, at the point where they have member B in common, etc. Each such distinct logical nexus may be considered as a "configuration." A configuration may also be defined as the structure of a conceptual scheme. In this manner, I define individuals A, B, etc., as unique and

idiosyncratic. At the same time, I specifically recognize that they all are the several members which a set of overlapping classes have in common. The several configurations pertaining to A, B, etc., as loci, where these classes overlap, define the unique personalities. The similarities between these configurations give a logical meaning to "basic personality."

We have carried this analysis in abstract terms sufficiently far to enable those interested in developing its implications to do so at their leisure. We must, at this juncture, turn to more practical things.

Each of these constructs, A.B., etc., is a unique thing, which does not mean, obviously, that it was brought about accidentally, but merely that it is unprofitable, *in the study of configurations, in terms of the frame of reference "configuration,"* to pursue in detail every causal chain, etc., to its remotest origins. In other words, I cannot understand the process whereby chromosomes got together to form "John Doe," *in terms of John Doe's personality*. I can understand it only in biological terms. What is of interest is the specific logical nexus between the various classes, and this nexus is "accidental" only in terms of each separate class. In concrete terms, the configuration, which is "idiosyncratic" in terms of biology or physics, etc., is not accidental, though unique, in terms of the frame of reference "configurations." To be once more rigorous, the conceptual scheme "personality of John Doe" has a meaningful structure.

On the other hand, it is inadmissible to view the *personality* of John Doe as the locus where such classes, *and* some basic personalities overlap. This is self-evident in terms of Russell's strictures on the meaning of "a typical Frenchman" or "Napoleon."

It must also be clearly understood that "the personality of John Doe" is epistemologically prior to "basic personality." Both are conceptual schemes, but of different types, the latter being more abstract than the former. It should be noted, furthermore, that the "basic personality" is not idiosyncratic in terms of the frame of reference "personality," in the sense that "personality" is idiosyncratic in terms of biology, physics, sociology, etc. This is an important point.

Hence, we must never view "basic personalities" as components of a unique personality, nor use them as explanations of the latter. If one is careful to use not merely the Kardiner-Linton conceptual scheme "basic personality," but the Kardiner-Linton technique of constructing

it, as well—which, according to Dr. Kardiner, many people fail to do—one can entirely avoid such erroneous procedures, which result in more or less disguised vicious circles.

In practice, it is very easy to distinguish, by a rule of thumb method, between personality and basic personality. It suffices to distinguish between expectations regarding the behavior of "any" policeman and expectations regarding the behavior of a person whom I know well. In the first instance, the set of expectations was formulated in terms of the fact that "any" policeman will define a given situation in the same way. In the second instance, the set of expectations was formulated in terms of what this well-known person will do in "any" situation. Substitute for policeman, "Frenchman," "great general," or any other meaningful term, and the rule of thumb method can be used as an adequate test of whether we are talking about a basic personality or a unique personality.

In logical terms, in the first instance, I consider a spatial ensemble, and consider the reactions of "basic personalities" to a constant situation. In the second instance, I consider a temporal ensemble and the reactions of a unique "personality" to variable situations.[30]

It is conceivable that, sooner or later, it might be found useful to draw the dividing line between psychology and sociology in some such manner. Psychology, in the strictest sense, wishes to know what John Doe does qua a unique person. Sociology, in the strictest sense, wishes to know what John Doe does qua social animal in general.

Culture and personality, as a discipline, is somewhat more specific than is sociology, and wishes to know what John Doe does qua American, or qua urban man, or qua judge, etc.

Consider the human species. Social scientists study men jointly in groups. Psychological scientists study the same men severally (Cf. the distinction between "all" and "every"). The former deal with a class of individuals, as a class. The latter, on the other hand, deal with a class of classes (each of the sub-classes containing one member). The fact that both of these broad classes overlap completely as regards membership, does not mean that they are the same. This is self-evident in class-calculus.

I cannot do more than mention one final problem which I have so far been unable to solve to my own satisfaction. I refer to the

[30] **Devereux, G.** The Social Structure of a Schizophrenia Ward and its Therapeutic Fitness. Journal of Clinical Psychopathology 6: 256–257. 1944.

problem of the exact nature of the nexus between the psychological and the sociological understanding of human behavior. Is this nexus the same as the one between the mechanical description of the movements of individual gas molecules vs. the statistical description of the volume of gas under study? Or is there a complementary relationship in Bohr's sense[31] between the psychological and the sociological understanding of human behavior? Or is there a new and different kind of nexus between the two? We do not know. Yet, sooner or later, this crucially important problem of all psychosocial sciences will have to be solved, if these sciences are to progress and are to be made useful to men and to mankind alike.

DISCUSSION OF THE PAPER[32]

Dr. Carl G. Hempel (*Queen's College, Flushing, N.Y.*):

The following comments on Dr. Devereux's stimulating lecture refer to three specific issues which seem to me to be of particular significance for the methodology of the social sciences.

1. One of Dr. Devereux's principal objectives was to clarify and delimit, for the purposes of sociological research, the meaning of the concepts of culture and of personality. There are two questions which arise in connection with his approach to this problem.

First, it seems to me that the concepts of culture and of personality are to the field of social studies what the concept of life is for the field of biology. The concepts in question determine the subject matter of the particular field of research. But in so far as this comparison is valid, it is important to note that biological research does not begin with an attempt to give a precise definition of the concept of life. Rather, it begins with an approximate idea as to what kinds of phenomena are to be included in this field of investigation, and fruitful research in biology can be carried out without any need for raising the question as to the nature of life. A full clarification of the concept of life stands, as it were, at the end, not at the beginning of biological research. Analogously, it seems to me, an attempt to set out, in the field of sociology, with a delimitation of the concepts of culture and of personality is not necessary and probably not even helpful for sociological research. Here again, the theoretical clarification of these concepts is one of the goals rather than the starting point of scientific inquiry.

The following additional observation might serve to amplify and supplement this point. In Dr. Devereux's lecture, various conceptions of culture and of personality, as suggested by different authors, were referred to. Since all of these were meant to be used in the scientific study of sociological problems, the question arises as to the criteria by means of which we may decide which of the alternative

[31] **Bohr, N.** Causality and Complementarity. Philosophy of Science **4**: 289–298. 1937.

[32] In fairness to Dr. Hempel, it should be stated that his comments were directed at an earlier version. If the published version no longer has certain shortcomings, it is due to the fact that I have accepted all of Dr. Hempel's suggestions and made the appropriate corrections in the manuscript. I am very grateful to Dr. Hempel for his illuminating and helpful comments. It might be added that the problem of the nexus between the "psychological" and the "sociological" understanding of behavior appears to have been solved by Allport's distinction between the "idiographic" and the "nomothetic." Allport's views expand, but do not change in any respect, the scheme presented hereinabove. (cf. **Allport, G. W.** The Use of Personal Documents in Psychological Science. New York. Social Science Research Council. 1942.)

conceptions ought to be accepted in preference to the others. The criterion which is used in scientific research is that of theoretical fruitfulness or of explanatory and predictive power. Of two alternative conceptions of some object of investigation, that one is preferable which makes possible the establishment of more comprehensive and more highly substantiated hypotheses or theories. Thus, e.g., the definition of the temperature scale and, hence, of the concept temperature has undergone several changes in the course of the development of thermodynamics. Each of these changes was made, not because the earlier definition was recognized to be "false" or "counterintuitive," but rather because, in terms of the modified concept, more comprehensive and accurate thermodynamical laws could be established. Analogously, when alternative conceptions of culture and of personality are considered, the question should be raised as to the specific hypotheses or theories that can be established in terms of them, so that the scope and the factual soundness of the latter may be investigated. Without specific reference to the way in which a proposed concept functions in some theory, its adequacy for scientific purposes cannot be judged.

2. Dr. Devereux suggested that the so-called complementarity of the wave and the corpuscle concept in the explanations of quantum phenomena in physics has an analogue in the relation between the psychological and the purely sociological explanation of a phenomenon in the field of sociology. This relation, as viewed by Dr. Devereux, is briefly this: The more precisely a sociological phenomenon is explained, and thus understood, in terms of the psychology of the persons concerned, the less precisely it is understood in terms of a purely sociological framework of explanation, and conversely.

It seems to me that the suggested analogy breaks down for the following reasons:

The complementarity principle in quantum physics is a consequence of the particular formal structure of a highly systematized and rigorously tested physical theory. To this theory, no parallel exists at all in contemporary sociology. In fact, I do not know of even a single case where a social phenomenon has actually been explained in terms of a purely psychological and also in terms of a purely sociological theory. Possibly, Dr. Devereux thinks of sociological explanation as an explanation by means of statistical laws concerning regularities in the behavior of large numbers of individuals, and of psychological explanation as consisting in the causal analysis of the behavior of every individual concerned; just as the results obtained in rolling dice may be explained in terms of the statistical laws of probability applied to long series of throws, or in terms of the causal laws of mechanics applied to every throw individually. But this situation is formally quite different from that obtaining in quantum physics, and if such alternative explanations can be achieved in the social sciences, no reason has been stated why they should be "complementary" to each other. The application of one type of explanation does not render the other impossible or unprecise.

3. Dr. Devereux repeatedly referred to the question of the independent existence of such entities as culture and personality. I fully agree with his opinion that this matter is irrelevant for the issues discussed in his lecture, but I would like to add that it is irrelevant for every conceivable scientific problem because it is devoid of theoretical meaning.

"Theoretical constructs," such as the concepts of gravitational field, or of neurotic personality, are used in science for the description of certain regularities in some specific field of research. Thus, for example, the assertion that the earth possesses a gravitational field is a short way of formulating certain physical laws concerning the behavior of falling bodies, pendulums, etc., in the neighborhood of the earth. To say that the earth possesses a gravitational field means that physical objects near the earth display certain regularities of behavior; and the question, whether "over and above" these empirically ascertainable regularities there exists, independently, a gravitational field, obviously involves a semantical confusion. Quite the same type of confusion underlies the question as to the in-

dependent existence of the culture of a group over and above its directly or indirectly observable manifestations. This point would hardly deserve being discussed here were it not for the fact that the existence of culture and personality as independent entities is sometimes thought to be an indispensable assumption, if the possibility of any kind of interaction between the two is to be understood. But this is a misapprehension. Such interaction can be described in statements to the effect that, in groups with certain specified cultural traits (i.e. patterns of group behavior), certain specified personality traits (i.e. patterns of individual behavior) occur reguarly or frequently. The question as to the existence of culture and personality as independent entities does not arise here at all. It is a pseudo-problem, capable, not of a solution by means of relevant scientific research, but only of dissolution by means of semantical analysis.

SECTION OF BIOLOGY

FEBRUARY 9 AND 10, 1945

Conference on *"Experimental Hypertension."*

The Section of Biology held a Conference on "Experimental Hypertension," as the fourth in the series for the Academic Year 1944–1945. Doctor William Goldring, New York University, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introductory Remarks," by William Goldring.

"Introductory Lecture on the Production and Pathogenesis of Experimental Hypertension," by Harry Goldblatt, Western Reserve University Medical School, Cleveland, Ohio.

"The Mechanism of Experimental Renal Hypertension," by Eduardo Cruz-Coke, Medical Faculty of the University of Santiago, Santiago, Chile.

"Renin and Nephrogenic Hypertension," by Luis Leloir and colleagues, University of Buenos Aires, Buenos Aires, Argentina.

"The Nature and Treatment of Experimental and Clinical Hypertension," by Irvine H. Page, Cleveland Clinic, Cleveland, Ohio.

"Experimental Chronic Hypertension: Its Mechanism and Amelioration by the Use of Various Blood Pressure Reducing Substances, by Arthur Grollman, Southwestern Medical College, Dallas, Texas.

"Treatment and Prophylaxis of Experimental Renal Hypertension with Renal Extracts and Marine Oils," by George E. Wakerlin, University of Illinois Medical School, Chicago, Illinois.

"Extraction Studies of Renin and of Tissue Substances Capable of Lowering the Pressure of Rats," by John Remington, University of Georgia Medical School, Augusta, Georgia.

NEW MEMBERS

Elected February 15, 1945

ACTIVE MEMBERS

Ambrus, Yolanda Valer, Ph.D., Physiological Chemistry. Assistant to Chief Chemist, Coty, Inc., New York, N. Y.

Armstrong, S. Howard, Jr., M.D., Physical Chemistry and Clinical Medicine. Associate in Medicine, Peter Bent Brigham Hospital; Instructor in Medicine and Research, Associate in Physical Chemistry, Harvard Medical School; Research Associate in Chemistry, Massachusetts Institute of Technology, Cambridge, Massachusetts.

Barth, Lester George, Ph.D., Associate Professor of Zoology, Columbia University, New York, N. Y.

Becker, George G., B.S., Biology, Psychology, Anthropology. In Charge of Plant Quarantine, U. S. Department of Agriculture, Hoboken, New Jersey.

Crosby, Eleanor J., M.S., Assistant Economic Geologist, Freeport Sulphur Company, New York, N. Y.

Cunningham, Raymond W., Ph.D., Pharmacology. Head of Pharmacology Research, Lederle Laboratories, Inc., Pearl River, New York.

Deans, Walter, Bacteriology and Serology. Chief Technician, Flushing Hospital, Flushing, New York.

Duggar, Benjamin Minge, Ph.D., LL.D., Plant Physiology. Consultant in Mycological Research and Production, Lederle Laboratories, Inc., Pearl River, New York.

Durfee, Charles H., Ph.D., Psychology. Director, Rocky Meadows Farm, Wakefield, Rhode Island.

Ecker, Paul Gerard, M.D., Internal Medicine. Interne, Peter Bent Brigham Hospital, Boston, Massachusetts.

Heyman, Karl, Ph.D., Organic Chemistry. Chief Chemist, Kearny Manufacturing Company, Kearny, New Jersey.

Holt, Arthur Melvin, Geology, Student, University of Texas, Austin, Texas.

Johnston, Thomas A., M.D., Parasitic Diseases and Radiology. Chief of Staff, Whitewell Hospital, Whitewell, Tennessee.

Kern, Charles J., B.S., Physical Chemistry, Chief Chemist. International Vitamin Corporation, New York, N. Y.

Kienle, Roy H., Ph.D., Physics and Chemistry. Assistant Research Director, American Cyanamid Company, Bound Brook, New Jersey.

Kleiner, Israel, S., Ph.D., Biochemistry. Professor, New York Medical College, New York, N. Y.

Kogbetliantz, Ervand George, D. Sc., Mathematics, Applied Geophysics, Astronomy. Professor of Mathematics, Ecole Libre des Hautes Etudes, New York, N. Y.

Lester, Charles Turer, Ph.D., Organic Chemistry. Assistant Professor of Chemistry, Emory University, Georgia.

Marks, Henry C., Ph.D., Biological Chemistry. Assistant Director of Research, Wallace and Tiernan Products Company, Belleville, New Jersey.

Nelson, Edwin E., Ph.D., Pharmacology. Director, Wellcome Research Laboratories, Tuckahoe, New York.

Piotrowski, Zygmunt Anthony, Ph.D. Associate in Psychology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Toone, Gilbert C., Ph.D., Organic Chemistry. Research Chemist, National Aniline Division, Allied Chemical and Dye Corporation, New York, N. Y.

Voet, Andries, Ph.D., Physics and Chemistry. Director of Research, J. M. Huber, Inc., New York, N. Y.

ASSOCIATE MEMBERS

Alper, Carl, M.S., Protein and Enzyme Chemistry. Acting Instructor in Chemistry, Tulane University, New Orleans, Louisiana.

Anderson, John A., Ph.D., Bacteriology, Nutrition. Associate Professor of Bacteriology, Rutgers University, New Brunswick, New Jersey.

Clark, Herbert Mottram, Ph.D., Physical and Inorganic Chemistry. Instructor in Chemistry, Sterling Chemical Laboratory, Yale University, New Haven, Connecticut.

Jandorf, Bernard J., Ph.D., Biochemistry. 2nd Lt., Sanitary Corps, Army of United States, Medical Research Laboratory, Edgewood Arsenal, Maryland.

Nicoll, Paul Andrew, Ph.D., Assistant Professor of Physiology, Indiana University School of Medicine, Bloomington, Indiana.

Seifter, Joseph, M.D., Pharmacology and Toxicology. Chief, Department of Pharmacology, Wyeth Institute of Applied Biochemistry, Philadelphia, Pennsylvania.

Stenzel, H. B., Ph.D., Geology and Paleontology. Geologist, Bureau of Economic Geology, University of Texas, Austin, Texas.

Wood, Scott E., Ph.D., Physical Chemistry. Assistant Professor of Chemistry, Yale University, New Haven, Connecticut.

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SECTION OF GEOLOGY AND MINERALOGY

MARCH 5, 1945

DOCTOR WALLACE M. CADY, Geologist, Alaskan Branch, United States Geological Survey, Washington, D. C.: *Aerial Photographs as an Adjunct to Arctic and Sub-Arctic Geologic Reconnaissance.** (This lecture was illustrated with lantern slides.)

The usefulness of aerial photography in the mapping of desert regions where bedrock is well exposed has been rather obvious for a long time. Geologic structures and more highly colored formations exposed in desert ranges are commonly registered in detail on aerial photographs and may be plotted directly on maps made from the photos, reducing the ground survey from a long, tedious, routine, mechanical procedure to a reconnaissance of critical localities guided by the photographs. In sub-arctic and arctic regions, aerial photos are particularly useful because, due to a heavy cover of tundra moss and frost-heaved ground, particularly in unglaciated regions, such as in the greater part of interior Alaska, geologic structures hard to recognize at close hand on the ground become visible from the air. In recent years, the U. S. Army Air Forces have photographed much of arctic and sub-arctic North America from the air, revealing geologic features previously unrecognized, although ground surveys may have already been made. Certain of the photographs released to the Geological Survey for official use have become a valuable adjunct to geologic reconnaissance in Alaska, undertaken primarily as an inventory of Alaskan mineral resources.

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Up to the time the aerial photographic surveys were begun, large areas amounting to at least half of Alaska remained unexplored. The photographic compilation has shown the distribution of drainage features in these hitherto unexplored areas, but ground surveys are still necessary to establish topographic and geologic control.

One of the larger unexplored areas in which the aerial photographs are being used in connection with ground surveys is the central Kuskokwim region, west of the Alaska Range and south of the Yukon River. Here the Kuskokwim River, second largest in the Territory, cuts westward in a relatively narrow gorge through the Kuskokwim Mountains and flows southwestward into the Bering Sea. The scattered higher peaks of the Kuskokwim Mountains, reaching altitudes averaging about 3,500 feet, have been maturely glaciated, but by far the greater part of the region is maturely dissected by streams forming well-rounded hills averaging about 2,000 feet in altitude with 500 to 1,000 feet of relief. The rounding and smoothing of the slopes have been accentuated by frost weathering. The climate is sub-arctic, i.e., the region lies south of the sea-level isotherm of 50° F. for the warmest month of the year. However, the hills and mountains above 1,000 feet altitude are bare of trees and the upland weather is comparable with that of the typical arctic tundra north of the warm-month isotherm of 50° F. and the poleward limit of forests. Spruce is the principal forest tree that covers the bottomlands. The light gray-green lichen, known as caribou or reindeer moss, covers the broad upland slopes.

The relatively wide expanses of upland tundra, cut by such deep narrow valleys as that of the Kuskokwim River, promote the optimum usefulness of aerial photographs as an adjunct to geologic reconnaissance. The bedrock features are best exposed in the cut-banks and bluffs along the main streams where frost action has not kept pace with stream cutting as it has on the uplands back from the streams. Here, small geologic structures of no greater magnitude than the length and height of the exposure may be made out, but, except possibly along the rather continuous gorge of the Kuskokwim River, the exposures are separated from one another by broad expanses of tundra, across which it is commonly difficult to connect the smaller structures through ground surveys alone.

The predominant bedrock formation of the region is a very thick succession of interbedded graywacke and shale of Upper Cretaceous age bordered on the northwest and southeast by Paleozoic formations.

The graywacke strata form faint ridges that are either entirely imperceptible on the ground or, as slope traces, are difficult to orient. These faint ridges and traces are readily distinguishable on aerial photographs taken at 20,000 feet. A variety of Tertiary igneous rocks, ranging from basalts to granites, is found in the region, although it is not as extensive as the graywacke and shale. Granitic rocks form stocks and cupolas with roughly circular groundplans; intermediate types form elliptical stocks and sills, conforming in orientation to structures in the graywacke-shale succession; and the basaltic rocks form thick surface flows or small tabular intrusives, principally sills. These igneous features are readily identified on the ground as they are rarely weathered down to the point where they are completely covered by tundra moss or foreign solifluction products. However, the granitic to intermediate intrusives, in particular, are recognizable on the aerial photographs because of their size and shape. Contact metamorphic zones are found in the graywackes and shales bordering the granitic stocks. The outer limits of these zones are about at the edge of the maturely glaciated areas among the higher peaks and thus are recognizable on the aerial photos. Talus slopes are developed much more extensively on the metamorphic rocks than on the granite forming the cores of the peaks, making it possible to tell the two apart.

Regional geologic structures are shown in more detail by the pattern of surface traces revealed on the aerial photographs than would be possible in standard ground-reconnaissance practice. Plotted photogrammetrically the traces form the basis of useful tectonic maps in areas where slope cover inhibits ground surveys. Thus far, ground surveys, particularly along river courses, have revealed rather tight minor folds in the Upper Cretaceous graywackes and shales. Major folds of greater extent than the individual exposures have been difficult to identify, due to the lack of good key strata that would assist structural interpretation in connection with either ground or aerial surveys. However, the trace pattern of areas between ground surveys, completed to date, suggests rather convincingly the orientation of fold axes, in general northeasterly, but with a tendency to parallel the contact of bordering Paleozoic formations, regardless of the orientation of the latter. Major high angle faults which have been passed over unnoticed on the ground are distinguishable on the photographs. The faults are particularly well developed along the borders of the Kuskokwim Mountains. Their nearly unbroken traces cross the bedding traces indicating that

the faults are later than the folds and probably related to a differential uplift of the Kuskokwim Mountains, following folding.

Of the various ore minerals prospected for and mined in the central Kuskokwim region, gold lies closest to granitic and intermediate igneous intrusives, such as are recognizable in the aerial photographs. Although lode gold has not been mined here, several groups of placer claims have been staked and operated in the vicinity of large rhyolitic sill-like bodies, certain of which are in close proximity to one of the granitic stocks. Tin and tungsten minerals are found in some of the placer concentrates with the gold. Quicksilver, the principal mineral product of the region, is found chiefly in or close to altered basic intrusives which have weathered deeply and are not in themselves large enough to be detected on the aerial photographs. The ore mineral, cinnabar, accompanied by varying amounts of stibnite, has been found in veins cutting the altered basic rocks and, to a lesser extent, in the other types of intrusives, except stocks of typical granites. The latter do not crop out in the areas in which cinnabar is found. Quicksilver metalization may be related to more deep-seated intrusives than those in their vicinity at the present surface, possibly parts of the granite, not yet unroofed, or deeper zones of the other intrusives. It is hoped that further study of the regional geology will help clear up this problem and thereby suggest guides in locating new quicksilver lodes in yet unexplored areas.

It is anticipated that, as an outcome of widespread aerial photographic surveys made in recent years, photogrammetric analysis of geologic features will soon become a very important preliminary to ground reconnaissance of unexplored regions in the search for new mineral reserves. As already stated, analyses of data obtained from aerial photographs of arctic and sub-arctic regions, such as those of Alaska, which, incidentally, constitute the greater portion of unexplored country, are particularly useful, because many geologic features seen in the photographs cannot be seen on the ground. It is hoped that simple, refined techniques and instruments will be developed to reduce surface trace lineations, however minute, to lines of known orientation from which the geologist can plot the dip and strike of planar structural elements for use in both tectonic maps and structure sections. Such analyses would be substantiated by ground examination of critical areas analogous to the ground control necessary for topographic mapping from aerial photographs.

SECTION OF BIOLOGY

MARCH 12, 1945

DR. ALEXANDER SANDOW, Assistant Professor of Biology, Washington Square College of Arts and Science, New York University, New York, N. Y.: *The Mechanism of Energizing Muscular Contraction.* (This lecture was illustrated by lantern slides.)

It is now generally agreed that the two most fundamental components of muscle are the fibrous protein, myosin, and the nucleotide, adenosinetriphosphate (ATP). Myosin, the principal component of the myofibrillae, is the contractile substance, while ATP, abundantly supplied with chemical potential energy in its two labile phosphate bonds, is the substance that directly energizes myosin for its mechanical activity. The basic problem of muscular contraction is to determine the mechanism by which the stored energy of ATP is transferred to myosin and converted by it into mechanical energy. In the discussion to be presented here, we shall limit ourselves to only the first phase of this problem, i.e., to the nature of the process by which the energy of the labile phosphate bonds of ATP is transferred to myosin.

A uniquely significant contribution to the solution of this problem is found in the discoveries of Engelhardt and his coworkers (1939, 1941, 1942) that (1) myosin acts as adenosinetriphosphatase, i.e., it is an enzyme, specifically catalyzing the hydrolysis of ATP to adenosinediphosphate (ADP) and free inorganic phosphate with the release of the 11,000 cal/mol PO_4 of the last phosphate bond of ATP; (2) artificially spun myosin fibers undergo an elongation in the act of splitting ATP. These findings are important, for they offer additional proof, if this be needed, that ATP is the direct energy source for myosin; and they suggest, furthermore, that the process by which the energy transfer is affected is a function of the enzymatic activity of the contractile substance and that this transfer is associated with the relaxation of myosin in some phase of the mechanical response of muscle.

One approach to the energizing problem is concerned with the temporal aspects of the interaction of myosin and ATP in relation to the sequence of latent, contraction, and relaxation periods that normally occur in a muscle twitch. Two general *a priori* possibilities exist. In the first, which we designate *activation coupling* (Ritchie's (1933) "Chemical" Theory), the transfer of energy to myosin occurs simultane-

ously with the direct activation of the contractile process in the latent and contraction periods; in the second, *recovery coupling* (Ritchie's "Physical" Theory), the energy transfer is affected during the relaxation period, and, more generally, probably also during some of the immediate post-relaxation period. In activation coupling, the stimulus applied to a muscle serves to initiate the interaction of myosin and ATP and this not only energizes myosin, but also activates it to develop tension. Thus, ATP would be not only the energizer of the contractile substance, but also the agent that directly stimulates it to contract. Recovery coupling, on the other hand, would involve the basic myosin—ATP reaction in the general post-contractile phase of a contraction, and it would serve essentially to transform the energy released by ATP hydrolysis into some form of "physical" potential energy of the myosin molecule. Subsequent stimulation would then act to bring about a discharge of the stored energy and this would lead to tension development. In the following discussion, several salient lines of research of various workers pertinent to the above will be analyzed. Some attention will then be given to certain of our own investigations.

The work of Ritchie (1933) was the first in recent years to pose the temporal problem in energization and to suggest a choice between the two possibilities. In conformance with the generally held view in the early 1930's, Ritchie assumed that creatinephosphate (CP) was the direct energy source for the contractile material. Since the rate of breakdown of this substance is decreased by increase in pH, he varied the pH of a muscle (the frog ventricle) and measured the corresponding changes in rate of activation and recovery processes in the response of this muscle. He found that the rate of activation, as measured by the duration of the latent period, was negatively correlated with the rate of CP hydrolysis. He, therefore, asserted that CP breakdown was connected with some recovery reaction, and thus concluded in favor of recovery coupling. (Ritchie's research also included experiments on the refractory period, but since this period is essentially connected with the excitatory aspect of muscle behavior, the results do not seem to be relevant to the present discussion, which is concerned with the contractile system. Incidentally, however, it is of interest to note that the variation in duration of the refractory period indicated that the speed of the recovery process implicit in this part of the excitatory reaction was found to be positively correlated with the rate of CP hydrolysis.)

Although Ritchie's experimental results are correct, their interpre-

tation today is faulty, since we know that ATP, and not CP, is involved in the direct energetic coupling of myosin. Now, in the physiological range of pH, varying the hydrogen ion concentration affects the rate of hydrolysis of ATP oppositely to that of CP. Thus, the rate of tension onset, as measured by the length of the latent period, is positively correlated with the speed of ATP hydrolysis. Hence, properly interpreted, Ritchie's results favor activation coupling.

Further evidence for activation coupling may be found in the important finding of Brown (1936) that extra tension in the isometric twitch of the turtle retractor penis muscle and the frog sartorius is induced by suddenly applied high pressure (272 atmospheres) only during the initial eighth of the contraction period. This result suggests that the highly pressure-sensitive process is a chemical reaction which is concerned with the mobilization of energy for contraction, and since this reaction occurs early in the contraction period, activation coupling would be involved.

Another source of evidence for activation coupling is found in the experiments of Buchthal and his co-workers (1944, a and b) which have demonstrated that, when ATP is directly applied to frog skeletal muscles or their isolated fibers and to mammalian smooth muscles, a contraction is initiated which involves, in all cases, a fall in birefringence of the activated muscle structures. Since "these experiments support the view that ATP is an essential agent in the release of normal muscular *contraction*" (my italics, A.S.), they lead to the conclusion that the energetic coupling of myosin and ATP occurs in the activation period.

Finally, as favoring activation coupling, there will be discussed the work of Dr. Joseph Needham and his coworkers (Dainty *et al.*, 1944). This research has demonstrated, among other things, that when a myosin solution is treated with ATP, decreases immediately occur in the flow birefringence and the relative viscosity of the myosin. These changes are relatively more slowly spontaneously reversed and, during this reversal, the hydrolysis of the ATP takes place. The initial physicochemical changes in the myosin indicate that the micellae of this protein shorten, in some manner, along the direction of their long axes. Thus, contact with ATP causes the myosin micellae to contract. It is noteworthy, however, that the actual splitting of the ATP is temporally associated with the disappearance of the shortened state. Therefore, it seems that the physical basis for contraction of the micellae may be the

formation of an enzyme-substrate combination between myosin and ATP.

Dainty *et al*, state, however, that further experiments "are still needed to decide between the two possibilities: (a) That the combination between myosin and ATP so alters the myosin molecule, that reaction between some groups along the length of the chain becomes possible and provides energy. The subsequent splitting off of phosphate from the substrate would then supply energy needed for relaxation and recharging of the myosin fibril. (b) That the splitting off of phosphate and setting free of energy from the ATP accompanies the contraction." It thus appears that the results of the Needham group cannot be unequivocally interpreted in all their aspects. But, nevertheless, it is, clear that ATP acts as an agent that causes myosin micellae to contract and, hence, in so far as the results of model experiments may be applied to the analysis of the contraction of live muscle, this finding points to a type of activation coupling mechanism.

Evidence will now be presented that seems to favor the recovery coupling mechanism. Kalckar (1941) calls attention to the very suggestive fact that, after sufficient activation of iodoacetate-poisoned muscles, there results, terminally, a mechanical condition of fatigue and rigor with which is associated a chemical state of extreme irreversible depletion of the ATP and CP stores. It is inferred that the inability to relax and contract, as indicated, respectively, by the rigor and the fatigue are the consequence of the absence of ATP and CP. Therefore, it is hypothesized that, in the normal muscle, which is rich in ATP and CP, relaxation occurs as a result of a coupling of these substances to myosin and that, during this process, the myosin is energized for a subsequent contraction. We may disregard the inclusion of CP in this process, since it can be only indirectly concerned with the energization of myosin by virtue of its role in the resynthesis of ATP. The mechanism proposed by Kalckar, however, is a form of recovery coupling. In criticism of this hypothesis, it may be mentioned that the rigor of iodoacetate fatigued muscles could be the result of metabolic disturbances, other than the depletion of ATP or CP, that are characteristic of the action of this drug. Furthermore, rigor does not necessarily accompany inability to contract, as is seen, for example, in completely fatigued normal muscles.

Engelhardt's finding (1941, 1942) that enzymatically active myosin threads suffer a relaxation has been used by him as a basis for the view

that the post-contractile relaxation of a contraction is the phase of a muscular response, during which myosin catalyzes the breakdown of its substrate, ATP. Thus, according to this view, the energizing of myosin for contraction is a function of the relaxation period. But Engelhardt was evidently unaware of the existence of a pre-contractile relaxation, the latency relaxation (LR) (Rauh, 1922; Sandow, 1944), which will be fully described later. If muscular relaxation is an *in vivo* sign of the coupling of myosin and ATP, then this interaction may be going on at the time of the LR. Hence, although Engelhardt interprets his experimental results so as to suggest a form of recovery coupling, an alternative interpretation which takes into account the LR, a latent period event, suggests that the energizing mechanism involves activation coupling.

It is evident that the investigations discussed thus far do not permit us to draw an indubitable conclusion concerning the mechanism of muscular energetic coupling. In so far as interpretations are based on experiments with purified extracts of myosin and ATP, we note that the Engelhardt view favors recovery coupling, while that of Needham group, in its most certain aspect, suggests a form of activation coupling. The contradiction between these two conceptions becomes more striking when it is realized that a basic experimental difference is involved, namely, that, despite the presence of a common specific myosin—adenosinetriphosphatase activity, Engelhardt's myosin fibers merely relax, while Needham's myosin micellae in solution first contract and then relax. The research with whole muscle presents a more unified picture. Reasons have been already given for questioning the validity of Kalckar's hypothesis. To these may be added the fact that his view is based on an abnormal—indeed, moribund—condition of muscles in iodoacetate rigor. We are thus left with the results of Ritchie, Brown, and Buchthal which favor activation coupling. Since these results were all obtained with quite normal live muscles, it would seem that this evidence is strongly weighted in favor of the activation coupling mechanism.

We shall now turn to our own investigations[1] which are concerned with studies of the latent period of frog skeletal muscle contraction, with particular reference to the latency relaxation (LR) first discovered by Rauh in 1922. A detailed description of the piezoelectric, cathode-ray oscillographic method of our experiments, and a general analysis of the

[1] Aided in part by a grant from the Penrose Fund of the American Philosophical Society.

LR will be found in Sandow (1944). An outline of these aspects of our research is given in Sandow (1945a). It will suffice here to state that the typical latent period at room temperature of a frog sartorius muscle, initially under a tension of 3 gms., begins, after the instant of application of the stimulus, with a mechanically quiescent phase having a duration, L_R, of about 1.0–1.5 ms. This is followed by a sigmoid relaxation phase, the LR, which, after achieving a magnitude, R, is abruptly reversed as the muscle, terminating the latent period, passes into its contraction period. The duration of the latent period, i.e., the time interval from the instant of stimulation to the first appearance of positive tension change, L_R, is about 3.0 ms. R is a negative tension change, generally of the order of one-thousandth of the positive tension output at peak of a twitch, and thus represents a tension decrease of about 20 mg. The precision of measurement of these events is generally of the order of $\pm$ 0.02 ms. for the time intervals and $\pm$ 0.1 mg. for the LR tension change.

The results[2] that will be discussed are concerned with the effects of pH and of temperature on the latency phenomena. The effect of pH changes, indirectly induced in the muscle by its own previous activity, is discussed in Sandow (1945, a and b). The general conclusion of this work, that the higher the pH, the shorter is L_T, has been confirmed by experiments (Sandow, 1943) in which the hydrogen-ion concentration of the muscle was directly modified over the range from pH 6.0 to 8.5 by exposing it to different mixtures of CO_2 and O_2 (or N_2) in bicarbonate-buffered Ringer's solution. Thus, we verify for skeletal muscle what Ritchie (1933) found for heart muscle. Since the tension latency, L_T, is shorter the higher the muscle pH, it is evident that the speed of some process directly determining the speed of tension onset is increased, the more alkaline the muscle. In view of the fact that the hydrolysis of ATP by myosin increases with pH (up to about pH 9.0) (Bailey, 1942; DuBois and Potter, 1943), we feel that this is the reaction occurring in the latent period that directly energizes and activates the rise of tension.

If the ATP hydrolysis that is supposed to take place in the latent period is specifically catalyzed by myosin, then it is important to search for independent proof of the involvement of the contractile enzyme. Evidence that myosin—in the sense of a fibrous protein—is involved in the formation of the LR is presented in Sandow (1944). But the in-

[2] These results were obtained in experiments performed in collaboration with Mr. A. G. Karczmar.

fluence of temperature, especially on R—the magnitude of the LR—indicates that the LR is a mechanical sign of the enzymatic activity of myosin.[3]

As the temperature is varied from 10° to 40° C., R increases to a maximum at about 24° and then falls at higher temperatures until, at 40°, it is only a few percent of the maximal value. If the muscle is kept for a minute or so at 40° and then quickly returned to the optimal temperature, 24°, the magnitude of R is greater than it was at 40°, but not as great as it was at 24°, before submission to the higher temperature. Thus, the inactivation of R at high temperature can be partially reversed. The detailed study of inactivation of R by high temperatures leads to the following conclusions: (1) The irreversible inactivation of R begins quite sharply at a critical temperature of about 37.5° and the rate of irreversible inactivation increases very rapidly within the range of the next few degrees. The temperature coefficient of this process at about 38°–39° expressed as a Q_{10} value is of the order of several thousand. (2) Another critical temperature exists at about 41°, for, at or above this temperature, completely irreversible inactivation of R occurs very quickly, i.e., in a few minutes.

The general variations of R with temperature are very similar to those characteristic of temperature effects on enzyme behavior. Therefore, it is inferred that the LR is an expression of the action of an enzyme. Furthermore, the two critical temperatures for the inactivation of R, one at 37.5°, the other at about 41°, are, respectively, essentially the same as those at which Mirsky (1937) has demonstrated the occurrence of primary and secondary heat denaturation of extracted frog myosin, and the high Q_{10} for the LR inactivation at 38–39° C. is comparable to that of the order of one thousand found at a similar temperature range for the primary denaturation process. The thermal features of the inactivation of the LR and the denaturation of myosin are so similar that we therefore conclude the LR is determined by some action of myosin. Since these generalizations concerning the significance of the temperature variations in R indicate that, on the one hand, the LR represents an enzyme action, and, on the other, a property of myosin, it is reasonable to infer that the LR is an indication of myosin acting enzymatically. This view is strengthened by the inference, derived by analysis of the effect of pH, that the hydrolysis of ATP—

[3] See Sandow (1943) for a preliminary announcement of the general effects of temperature on the latency phenomena. Only the behavior of R will be discussed here.

the substrate of myosin—is occurring during the latent period which includes the time interval of the LR.

The synthesis of the results of our pH and temperature experiments thus indicates—inferentially, it is true—that the latent period is a time interval during which myosin acts enzymatically on ATP. Indeed, if Engelhardt had not discovered the ATP-ase activity of myosin, our experiments might have been used to suggest this as a possibility. But, in view of Engelhardt's finding, the unique contribution of our research lies in the suggestion that, in the live muscle, the enzymatic interaction of myosin and ATP, and hence the energizing of myosin, take place during the latent period of the mechanical response. Thus, our work lends support to the view that muscular contraction is energized by the mechanism of activation coupling.

The question that now arises is the detailed nature of the coupling of ATP and myosin, and its relation to the sequence of mechanical changes in a stimulated muscle, first relaxation—the LR—and then, contraction. The following theory (preliminary notice in Sandow, 1943) is proposed in explanation of these events. Assume that the reaction between myosin and ATP in stimulated muscle involves the formation of an intermediate enzyme-substrate combination in accordance with the following scheme:

$$M + ATP \longrightarrow M\text{-}ATP \longrightarrow M_C + ADP + H_3PO_4 \qquad \text{(I)}$$

in which M represents myosin at rest, M-ATP, the intermediate combination, M_C, contracting myosin, and ADP, adenosinediphosphate. This scheme pictures the transfer of energy from ATP to M as a function of the intermediary, M-ATP; and when this transfer is accomplished, there are released from the intermediary, not only the energy-degraded products of the hydrolysis of ATP, but also the energy-enriched and contracting myosin, M_C. Since M corresponds to the muscle at rest and M_C to contracting muscle, it is therefore supposed that the intermediate M-ATP is the form of myosin responsible for the LR that intervenes between rest and contraction. In other words, we specifically assume that the LR is a mechanical sign in live, stimulated muscle of the presence of the enzyme-substrate combination, M-ATP.

In addition to evidence already adduced in support of our assumption concerning the significance of the LR, we may point to the result of Engelhardt that myosin threads elongate in association with their enzymatic activity. It is possible that this elongation corresponds to

the formation of an M-ATP complex. It is true, in the Engelhardt experiment, that, even though the ATP was hydrolyzed, the myosin threads remained extended; but the failure to contract may be a consequence of the processes of extraction and formation of the myosin into threads. Be that as it may, our assumption is in direct contradiction to the view of the Needham group that *contracting* myosin is represented by the M-ATP combination. But this view is an inference. Furthermore, it is based on experiments with myosin in solution and not in the form of fibers; and, as Dainty *et al.* (1944) state, "It must be remembered that, *in vitro,* the contraction of the myosin particles does no work, and the conditions are therefore very different from those *in vivo.*" Therefore, it does not seem to be too arbitrary to adopt the contrary assumption concerning the role of M-ATP that we have included in our theory. Finally, the possibility should be mentioned that the behavior of myosin in live muscle may represent a synthesis of the mechanical behavior of extracted myosin found in the Engelhardt and Needham experiments; i.e., the LR corresponds to the elongation of Engelhardt's myosin fibers, and the subsequent contraction and relaxation are equivalent to the shortening and then lengthening of the Needham micellae.

Since, in our theory, M-ATP is the LR form of myosin, we will designate it by M_R. If it be now assumed that the reactions, $M \rightarrow M_R$, and $M_R \rightarrow M_C$ are, in first approximation, monomolecular, with velocity constants k_1 and k_2, respectively, then the determination of the course of the mechanical change of the stimulated muscle reduces, in the first place, to the problem of the kinetics of the following chain of reactions:[4]

$$M \xrightarrow{k_1} M_R \xrightarrow{k_2} M_C. \tag{II}$$

M_R and M_C will not exist in the unstimulated muscle. Hence, taking the initial concentration of M equal to unity, we have, as the initial conditions for Reaction (II) (using small m's to represent concentrations): $m = 1$, $m_R = m_C = 0$. The solution of this system giving the m's as a function of time, t, is well known:

[4] A formally identical reaction system, but with different meanings attributed to the reactants, has been proposed by Gilson, Walker, and Schoepfle (1944) as a theoretical basis for the course of the tension output in a muscular contraction. The relation between this theory and ours will be discussed elsewhere.

$$m = e^{-k_1 t}, \tag{1}$$

$$m_R = \frac{k_1}{k_2 - k_1}\,(e^{-k_1 t} - e^{-k_2 t}), \tag{2}$$

$$m_C = 1 - \frac{k_2}{k_2 - k_1}\,e^{-k_1 t} + \frac{k_1}{k_2 - k_1}\,e^{-k_2 t}. \tag{3}$$

Thus, the tension development of the muscle; i.e., the change from the resting state of M, will be given by some function of the quantities m_R and m_C. Now, for large values of the time, $m_R \rightarrow 0$, and $m_C \rightarrow 1$. Thus, for $t = \infty$, all the myosin is in the contracted state, and this, it is assumed, will correspond to the time of maximum tension in a tetanus. For simplicity, set this maximum tension equal to unity.

At present, there is no basis for choosing a particular form of the function of m_R and m_C to represent the tension development. With a first approximation in mind, however, we will make the assumption that the tension, T, of the muscle, at any instant, is given numerically by the difference between m_C, the concentration of the contracted myosin, and m_R, that of the relaxed intermediary myosin. Hence,

$$T = m_C - m_R \tag{4}$$

and, using the expressions given in Equations (2) and (3),

$$T = 1 - \frac{k_2 + k_1}{k_2 - k_1}\,e^{-k_1 t} + \frac{2k_1}{k_2 - k_1}\,e^{-k_2 t}. \tag{5}$$

Omitting the analytical details here, a first study of Equation (5), in comparison with experimental curves for the time-course of the usually recorded isometric tension production in a tetanus, and for the latency relaxation of a frog sartorius muscle at about 20° C., leads to the choice of the values: $k_1 = 20$, and $k_2 = 800$. Inserting these values in Equation (5), we have,

$$T = 1 - 1.0513\,e^{-20t} + 0.0513\,e^{-800t} \tag{6}$$

A plot of this equation shows that the tension curve is strikingly similar to corresponding experimental myograms of a tetanus. But of greatest interest is the fact that Equation (6) includes an initial phase involving a very slight and short negative tension charge which is the theoretical equivalent of the actual latency relaxation. The theoretical LR, however, differs in several respects from the actual. The theoretical curve begins at the zero of time, while the actual latency relaxation does not begin, in general, until about 1.5 ms. after the instant of stimulation. This merely corresponds to the fact that, in the muscle,

some reaction concerned with the excitatory process must run its course before resting myosin begins to be transformed into relaxing myosin. This process may be, in part, the release of Ca that Bailey (1942) postulates acts *in vivo* to activate myosin-ATP-ase. There are other deviations of the theoretical curve from the experimental, but these are of secondary importance, and, at any rate, are too detailed to take up here.

The fact that the theory of muscular contraction, outlined above, leads to an overall tension curve that begins with an initial decrease, followed by an increase of tension that agrees in certain respects quite closely with the actual behavior of a muscle tetanus, is presumptive evidence in support of the assumed underlying mechanism. Further corroboration for this view may be found in several other considerations, such as, for example, the possibility that the variation of m_R with time as expressed in Equation (2) provides a definite basis for Brown's (1941) alpha-process. But such discussion must be postponed for the full-length publication of our theory.

It must be stressed that the present discussion deals with the results merely of a preliminary analysis of the basic conceptions symbolized in the schemes of Reactions (I) and (II). As more work is done on this theory, modification in detail will undoubtedly have to be made. Furthermore, the theory deals only with the events of the latent and contraction periods of muscular response. Post-contractile relaxation is not considered. In this connection, it is significant that the theory is deliberately limited in the sense that it does not include some process that restores contracted myosin to its original rested state during this relaxation phase. However, it may be possible to extend the theory so as to make it conform with the view, for which some evidence exists, that special chemical reactions during post-contractile relaxation actively serve to transform contracted into resting myosin.

Finally, it is evident that the theory we have outlined is a particular formulation of the mechanism of activation coupling, and, therefore, in so far as it has merit, it upholds this conception of the energization of muscular activity. Quite apart from this theory and its connotations, however, the general conclusions and inferences of our experiments on the effects of pH and temperature on the latent period of contraction provide independent support for activation coupling comparable to that of Ritchie (1933), Brown (1936), and Buchthal (1944 a and b), previously discussed. Thus, it is clear, in view of the particular

activation mechanism our theory provides, and especially because of the experimental evidence of our own and other research dealing with live muscle, that there is good reason for concluding that activation coupling is the means by which muscular contraction is energized.

LITERATURE CITED

Bailey, K.
1942. Myosin and adenosinetriphosphatase. Bioch. J. **36**: 121-139.

Brown, D. E. S.
1936. The effect of rapid compression upon the events in the isometric contraction of skeletal muscle. J. Cell. and Comp. Physiol. **8**: 141-157.

Brown, D. E. S.
1941. The regulation of energy exchange in contracting muscle. Biological Symposia **3**: 161-190.

Buchthal, F., A. Deutsch, & G. G. Knappeis
1944. Adenosinetriphosphate initiating contraction and changing birefringence in isolated cross striated muscle fibres. Nature **153**: 774.

Buchthal, F., & G. Kahlson
1944. Application of adenosine-triphosphate and related compounds to mammalian striated and smooth muscle. Nature **154**: 178-9.

Dainty, M., A. Kleinzeller, A. S. C. Lawrence, M. Miall, J. Needham, D. M. Needham, & Shih-Chang Shen
1944. Studies on the anomalous viscosity and flow-birefringence of protein solutions. III. Changes in these properties of myosin solutions in relation to adenosine-triphosphate and muscular contraction. J. Gen. Physiol. **27**: 355-399.

DuBois, K. P., & V. R. Potter
1943. The assay of animal tissues for respiratory enzymes. III. Adenosinetriphosphatase. J. Biol. Chem. **150**: 185-195.

Engelhardt, W. A. & M. N. Ljubimova
1939. Myosin and adenosine-triphosphatase. Nature **144**: 668-669.

Engelhardt, W. A., M. N. Ljubimova, & R. A. Meitina
1941. Chemistry and mechanics of muscle studied on myosin. Compt. Rend. Acad. Sci. U. R. S. S. **30**: 644-6 (as abstracted, 1943. Chem. Abstr., **37**: 391-2).

Engelhardt, W. A.
1941. Enzymatic and mechanical properties of muscle proteins. Advances in Contemporary Biology **14**, edition 2.
1942. The above article reprinted in translation. Yale J. of Biol. and Med. **15**: 21.

Gilson, Jr., A. S., S. M. Walker, & G. M. Shoepfle
1944. The forms of the isometric twitch and isometric tetanus curves recorded from the frog's sartorius muscle. J. Cell. and Comp. Physiol. **24**: 185-199.

Kalckar, H. M.

1941. The nature of energetic coupling in biological systems. Chem. Rev. **28**: 71-178.

Mirsky, A. E.

1937. Contraction of muscle and denaturation of myosin. Proc. Soc. Expt. Bio. Med. **37**: 157-159.

Needham, J., A. Kleinzeller, M. Miall, M. Dainty, D. M. Needham, & A. S. C. Lawrence

1942. Is muscle contraction essentially an enzyme-substrate combination? Nature **150**: 46-49.

Rauh, F.

1922. Die Latenzzeit des Muskelelementes. Zeit. f. Biol. **76**: 25-48.

Ritchie, A. D.

1933. Theories of muscular contraction. J. Physiol. **78**: 322-334.

Sandow, A.

1943. Study of the effect of pH, tissue poisons, and anistonicity on the mechanical events of the contraction and relaxation periods of skeletal muscular contraction. Year Book of the Amer. Philo. Soc.: 195-198.

1944. Studies on the latent period of muscular contraction method. General properties of latency relaxation. J. Cell. and Comp. Physiol. **24**: 221-256.

1945a. The effect of activity on the latent period of muscular contraction. (Abstract) Trans. N. Y. Acad. Sci. (2) **7**: 78-80.

1945b. The effect of activity on the latent period of muscular contraction. Ann. N. Y. Acad. Sci. **46**. In press.

SECTION OF PSYCHOLOGY

March 19, 1945

Doctor Edna Heidbreder, Professor of Psychology, Wellesley College, Wellesley, Mass.: *The Attainment of Concepts—A Psychological Interpretation.*

(An abstract of this paper will be published in the May issue of the Transactions.)

SECTION OF ANTHROPOLOGY

March 26, 1945

Doctor A Kardiner, New York, N. Y.: *Psychological Criteria for an Intercultural Comparison of Personality.*

(No abstract of this paper has been received.)

SECTION OF PSYCHOLOGY

MARCH 30 AND 31, 1945

Conference on *"Non-Projective Personality Tests."*

The Section of Psychology held a Conference on "Non-Projective Personality Tests," as the fifth in the series for the Academic Year 1944–1945.

The program consisted of the following papers:

PERSONALITY INVENTORIES

Chairman, Anne Anastasi, Queens College, Flushing, New York.

"Recent Research with the Multiphasic Personality Inventory," by G. Hamilton Crook, Personnel Research Section, Classification and Replacement Branch, The Adjutant General's Office, New York, N. Y.

"The Effect of Alcohol on the Personality Profile (Minnesota)," by Major Harold A. Abramson, (M.C.) A.U.S.

"Psychological Screening Procedures in the War Shipping Administration," by Sanitarian (R), George G. Killinger, Chief, Psychological Service, U.S.P.H.S., and Past Assistant Sanitarian (R), Joseph Zubin, Chief, Medical Statistician, Psychobiological Program, U.S.P.H.S., War Shipping Administration.

THE CORNELL INDEX AND THE CORNELL MULTIPLE CHOICE WORD ASSOCIATION TESTS

Chairman, Henry E. Garrett, Columbia University, New York, N. Y.

"Construction and Standardization," by Bela Mittelmann and Keeve Brodman, Cornell University Medical College, New York, N. Y.

"Results," by Arthur Weider, Cornell University Medical College, New York, N. Y., and David Wechsler, Psychiatric Division, Bellevue Hospital, New York, N. Y.

"Application," by Harold G. Wolff, Cornell University Medical College and New York Hospital, New York, N. Y.

"The Cornell Selectee Index as an Aid in Psychiatric Diagnosis," by Lt. Comdr. Harold J. Harris, (MC.) U.S.N.R.

Ability Patterns and Personality

Chairman, David Shakow, Worcester State Hospital, Worcester, Massachusetts.

"The Expression of Personality and Maladjustment in Intelligence Test Results," by Roy Shafer, Research Psychologist, Menninger Clinic, Topeka, Kansas.

"Personality Diagnostic Evaluation by Means of Non-projective Techniques," by Edith Wladkowsky, Psychiatric Division, Bellevue Hospital, New York, N. Y.

"Difference between Cases with Inventories Giving Satisfactory and Unsatisfactory Results," by Zygmunt A. Piotrowski, Psychiatric Institute, New York, N. Y.

Theory

Chairman, Gardner Murphy, College of the City of New York, New York, N. Y.

"Hypotheses Underlying Non-Projective Tests of Personality," by David Rapaport, Head of Department of Psychology, Menninger Clinic, Topeka, Kansas.

Problems of Performance Analysis in the Study of Personality," by Martin Scheerer, College of the City of New York, New York, N. Y.

ANNOUNCEMENT OF PUBLICATIONS FOR 1945[1]

The following publications will be issued by the Academy during the current year. Members of the Academy who desire to receive these papers will kindly request the Executive Secretary to send them, and they will be mailed, free of charge as they are ready for distribution, except as qualified in the footnotes:[2]

Annals

1. "Diffusion of Electrolytes and Macromolecules in Solution." Papers delivered at the conference by this title. (Approximately 146 pages.)
2. "Animal Colony Maintenance." Papers delivered at the conference by this title. (Approximately 130 pages.)
3. "The Effect of Activity on the Latent Period of Muscular Contraction," by Alexander Sandow. Awarded an A. Cressy Morrison Prize, 1945. (Approximately 30 pages.)
4. "A Hitherto Undemonstrated Zoogleal Form of *Mycobacterium tuberculosis*," by Eleanor Alexander-Jackson. Awarded an A. Cressy Morrison Prize, 1945. (Approximately 20 pages.)
5. "Respiration and Germination Studies of Seeds in Moist Storage," by Lela V. Barton. Awarded Honorable Mention, A. Cressy Morrison Prize Competition, 1945. (Approximately 20 pages.)
6. "Surface Active Agents." Papers delivered at the conference by this title. (Approximately 157 pages.)

Special Publications

Volume III

"Experimental Hypertension." Papers delivered at the conference by this title. (To be issued as a cloth bound volume, which will be available at a special rate for Members of the Academy.)

Transactions

Series II, Volume 7, Nos. 1–8. These papers are sent to all Members of the Academy, regularly, during the Academic Year. (Approximately 200 pages.)

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[1] Notice of additional publications to be added to this list will be sent to Members later.
[2] Active, Sustaining, Life, and Honorary Members may receive, upon request, a copy of all current numbers of the Annals.
Associate and Student Members are entitled to receive one complete monograph, or up to 150 pages of smaller papers.

NEW MEMBERS

Elected March 22, 1945

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY*

APRIL 2, 1945

DOCTOR WILLIAM C. KRUMBEIN, Beach Erosion Board, War Department, Washington, D. C.: *Sedimentary Maps and Oil Exploration.*† (The lecture was illustrated by lantern slides.)

Within the past two decades, advances in sedimentation have been very rapid. Techniques for the quantitative measurement of sedimentary attributes have been developed; the geological significance of particle properties is better understood; lateral variations in sedimentary environments are being studied; and a body of theoretical knowledge is being made available for applied geological problems. As the more theoretical or academic studies are translated into general geological terms or into general principles of sedimentation, they become increasingly valuable to the stratigrapher, the paleontologist, and the petroleum geologist.

Present trends in sedimentation are unquestionably toward a more quantified approach, and sedimentary attributes are increasingly expressed as numbers. Such numbers, having various degrees of statistical significance, have already been applied to particle properties, as size, shape (sphericity), roundness, and others; to mass sediment properties, as porosity, permeability, and the like; and to various chemical and mineralogical properties. The numbers are valuable in describing and comparing sediments, but their greatest practical significance is

* No meeting of the Section of Biology was held in April.
† This lecture was delivered under the title "Modern Sedimentation and the Search for Petroleum." The written version emphasizes certain portions only.

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Executive Secretary: Eunice Thomas Miner.
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realized when they are used to present the areal variation of the attribute. In response to this need, the quantitative trend has been paralleled by the development of maps to show the areal distribution of sediment properties. Maps of sediment thickness (isopach maps) are in common use, and maps of particle size variation, average permeability, organic content of the sediments, and the like, are beginning to appear.

As soon as numerical sedimentary data become available over an area, a contour-type map of the attribute may be made by drawing lines of equal value through the field of numbers scattered over the area. These contour-type maps are an outgrowth of the quantitative approach, and the development of additional maps of the same sort depends upon further extensions of the quantitative method.

Current interest in the development and use of new maps to solve stratigraphic and structural problems is illustrated by Kay's recent paper (1945), in which a classification is presented. The contour-type sedimentary map, properly referred to as an *isopleth map*, is classified by Kay under his paleolithologic group. However, in terms of construction, isopach maps and some paleogeographic maps belong to the contour type. Thus, the present discussion cuts across Kay's classification to some extent, and indicates merely that it is based upon use, whereas the present treatment is based upon similarity of construction and geometrical properties.

The necessary data for constructing contour-type maps include values of a given variable (average size, thickness, permeability, etc.), each associated with a specific geographic and stratigraphic location. The known values must be spaced closely enough over the area to bring out, at least, the overall trend in the data.

The geometrical properties of all such maps are the same as those of an ordinary contour map, and the same geometrical principles of interpretation can be used, with due allowance for the difference in the physical attribute shown on the map. For the most general case of a contour-type map, if the numbers represent elevations above sea level, it is an ordinary contour map; if they represent thickness of sediment, it is an isopach map; if they represent average particle size, it is a size map; and so on. The important point is that, in each instance, the lines are lines of equal magnitude or quantity, and they are related to their neighbors by some constant difference, product, or power. Hence, each map represents a surface, with the spacing between the lines in-

versely proportional to the slope of the surface. These surfaces have certain geometrical properties also. If the surface is an ordinary landscape, rainwater falling upon it runs down-slope at right angles to the contours, because a line normal to the contour, at any point, is the direction of steepest slope at that point. By drawing a series of normals on the surface or its map, lines of maximum slope are obtained, superimposed on the lines of equal magnitude. For a land surface, these lines of maximum slope indicate the directions of surface runoff; for other sediment properties, the lines may also have a physical significance. For example, if the surface represents a size map, the directions of most rapid change of permeability follow upslope along the slope lines. This relation follows from the fact that permeability increases as the square of the average diameter, and the diameters increase most rapidly upslope. Thus, it would be possible, in some instances, to construct a permeability surface from the size surface. In fact, it is very likely that the several surfaces representing various dynamic attributes of the sediment would all be very similar, and differ mainly in steepness and height.

It appears to be possible, also, to apply potential theory to such maps or surfaces by considering the contour lines as lines of equal potential, and the lines of maximum slopes as streamlines. To the extent that the theory is applicable to some or all of the maps, the whole background of reasoning associated with potential theory may be woven into the map interpretation. Restrictions will depend upon the variable plotted on the map, and whether such variable satisfies the conditions necessary to establish the existence of a potential function.

The significant point in the extension of contour-type maps to sedimentary data is that, although the preparation of the maps may require skill in sedimentary technique, the interpretation of the maps follows standard lines of geometrical reasoning. Hence, the general geologist, skilled in map interpretation, may find much of value from the maps in conjunction with other geological data bearing on his particular problem. Just as a series of principles has been developed for the interpretation of contour maps of land surfaces, so may a similar set or sets be developed for the study of sedimentary maps. In using such maps to narrow down exploration possibilities for stratigraphic traps, it may well be that, instead of the closed contour which did yeoman service in the search for structural traps, particular groupings of the sedimentary contours may be significant. Where rapid changes in the sedi-

mentary attributes result in steepening of the surface, they may indicate that other features are changing rapidly also. In short, it may be the steeper slopes or the margins of steeper slopes that are significant in the search for oil.

Contour-type (isopleth) maps of sedimentary data range from general features presenting an overall picture to detailed maps of specific attributes. A stratum of sandstone may be converted into values of thickness, mean size, sorting, heavy mineral content, permeability, etc., and each of these used as the basis for a contour-type map. Such maps are factual in that they present the observed areal variation of a chosen variable, but more or less interpretation is necessarily included, because incomplete data may require interpolation or extrapolation.

The following classification shows the range of possibilities in contour-type maps of sediments. The classification is not to be considered a rigid system. The groupings are made to suggest family relations among the maps, rather than as categories into which a map either falls or does not fall. Examples are cited in each group, but the particular literature references are mainly illustrative. Some of the data have never been presented in map form so far as the writer is aware, but their unquestioned inclusion in the general classification suggests their usefulness for shedding additional light on sedimentary problems.

1. Maps Based on Overall Relations. This group includes general maps which indicate overall or regional features. Examples are regional stratigraphic maps based on lines of equal percentages of shale, sand, or lime; structure contour maps (as indicating the attitude of the sedimentary beds); maps of facies changes; and various paleogeographic maps, such as paleogeomorphic maps, providing the ancient land surfaces can be shown by contours. Published examples include a facies change map by Hale (1941), and a paleogeomorphic map by Jager (1942).

2. Maps Based on Particle Properties. Here is included a large group of maps which show the areal variation of measured attributes of the sedimentary particles. The attributes, size, shape (sphericity), roundness, surface texture, and orientation (petrofabrics) are generally recognized as the more significant (Mineral properties are treated separately below). These maps may indicate, not only average values of the particle attributes, but also a host of other statistical parameters. Size attributes, for example, may include maps of average size, degree of sorting, skewness, kurtosis, ratios of size to sorting, and so on.

These several examples may be repeated for all the particle properties. Surface texture, at present, cannot be expressed numerically, although lines of equal percentages of frosted or polished grains can be shown. Maps of size attributes are most common. Pye (1944) shows the size variation of the Bethel sandstone; a sorting map was published by Krumbein and Aberdeen (1937); and a skewness map is shown in Krumbein and Griffith (1938).

3. MAPS BASED ON MASS PHYSICAL PPOPERTIES. This group includes properties of the aggregate, such as porosity, permeability, color, average density, thickness (isopach maps), and others. If the data represent a single stratum or horizon the maps may be geographic. Averages over thicker sections may include the time element and thus be stratigraphic. Some geophysical maps are included here, to the extent that they indicate average physical properties. Published examples in this group include many isopach maps (see Lee, 1943, for example). A permeability map is shown in Headlee and Joseph (1945).

4. MAPS BASED ON MASS CHEMICAL PROPERTIES. Many maps are possible in this group, inasmuch as almost any measurable chemical attribute may be included. Examples include the percentage of a given element within a stratum, or of a particular compound. Various ratios, such as the Ca/Mg ratio or its reciprocal, may be useful in indicating changes from limestone to dolomite, with all the implications which accompany such a change. Maps of insoluble residues, of degree of cementation, of percentage of organic carbon, of radioactive properties, are all included here. Trainer (1932) published an insoluble residue map. Krumbein and Caldwell (1939) show a map of organic content of tidal lagoon sediments.

5. MAPS BASED ON MINERALOGICAL PPOPERTIES. Heavy mineral data lend themselves well to presentation on contour-type maps. The percentages of heavies, or the percentages of various species, or ratios among various species (say non-resistant to resistant) may yield interesting information. The cementation maps mentioned under (4) may be included here, if presented as calcite vs. quartz cement, for example. Smithson (1939) plotted certain diagenetic minerals to study the effect of post-depositional changes on mineral distribution patterns. A map showing percentage variations in heavy minerals was published by Caldwell (1940).

6. MAPS OF ASSOCIATED GEOLOGICAL PROCESSES. Energy conditions within an environment may be shown by lines of equal energy

dissipation. Maps of current distribution, velocity variations, and so on, may all be valuable for indicating relationships to sediment attributes. For example, a map of the detailed variations in flow parameters of a stream, related to a map of sediment size, may account, in part, for the observed rapid areal variations in stream deposits. Maps such as these are necessarily confined to modern sedimentary environments, but, in some instances, they may be reconstructed from ancient sediment patterns. Maps of wave refraction give evidence of energy flow to a shore. Johnson (1919) shows an example.

7. Maps of Associated Fauna and Flora. Many biologic and paleontologic features can be presented on contour-type maps. Examples which suggest themselves include lines of equal population density (number of fossils per unit volume), based on whole faunas or particular species. Maps of variations in faunal assemblages, expressed numerically as percentages or otherwise, from fresh water to marine in an estuarine environment, would be very instructive in indicating degrees of variation and overlaps. Such maps would prove useful in establishing principles of correlation across facies changes. The writer is not aware that maps in this group have been published.

8. Maps of Associated Fluids. The fluids contained within the sediments constitute part of the sedimentary or post-depositional story, and are appropriately considered in this classification. Percent saturation of oil, water, or gas, are examples. The A.P.I. gravity of the oil, the salt content of contained water, the chemical constituents of oil or gas, are all subjects which yield instructive and useful maps. Bottom-hole pressures, initial yields, and other associated phenomena are mapped and extensively used for engineering and reserve-estimate purposes. It is unnecessary to emphasize that such maps prove helpful in solving sedimentary problems also. Numerous published examples of this group are available. Bottom-hole pressure maps are shown by Weeks and Alexander (1942); Plummer and Sargent (1931) showed lines of equal salt-content of associated waters; and Price and Headlee (1942) have a map of the thermal value of natural gas.

9. Maps Based on Combinations of Attributes. Combination maps afford an additional group which may be important in some studies. Here are included combinations based on sums, differences, products, or quotients of two or more variables. An example is a map based on the product of porosity and thickness (the "isovol" map of Weeks and Alexander, 1942). Torrey (1934) showed percentage of

cement on a map of initial well production, which expresses the combination idea, although no ratio or product was used. Other possible maps are based on non-dimensional ratios, such as average particle size divided by thickness; or permeability in darcys divided by thickness squared. Such combination maps may serve to bring into bolder relief contrasts or similarities among attributes, thus focussing attention on particular localities.

In addition to the preparation of maps which combine sediment properties, it is instructive to compare several individual maps of a given area. An isopach map may show some relation between thickness and known occurrence of oil, but the data are strengthened if maps of cementation, permeability, average particle size, and others, are also available. In short, in sedimentation, as in all other aspects of geology, it is the convergence of evidence which is important. The writer has suggested that it would be profitable to examine a known petroliferous area or stratigraphic pool by plotting the known occurrence of oil on a whole series of sedimentary maps to detect some of the factors which may have controlled the localization of the oil. Although such a study may be largely historical in terms of oil, two possible byproducts may be the discovery of additional prospects in the same area and a set of principles which could be used in exploring new territory.

Perhaps the main point to be emphasized in this presentation is that there exists a large family of maps of sedimentary attributes, many of which have already demonstrated their usefulness. The writer submits the thesis that other, as yet unmapped attributes, may prove equally useful by reflecting the dynamic conditions of sedimentation or significant post-depositional changes. It is not possible, in the present state of knowledge, to predict which particular maps will be most significant in a given situation.

The writer does not imply that sedimentation alone affords the key which will unlock the door to stratigraphic traps, but he does insist that there remain many lines of attack which have the known merit that they will shed additional important light on sedimentary problems. To the extent that such new light is integrated with other lines of evidence, it seems not unlikely that the forward movement of quantitative sedimentation, stratigraphy, paleontology, and structural geology can all contribute to a renaissance of geological methods for finding stratigraphically-trapped oil.

LITERATURE CITED

Caldwell, L. T.
1940. Areal variations of calcium carbonate and heavy minerals in Barataria Bay sediments, Louisiana. Jour. Sed. Pet. **10**: 58–64.

Hale, Lucille
1941. Study of sedimentation and stratigraphy of Lower Mississippian in Western Michigan. Bull. Amer. Assoc. Petrol. Geol. **25**: 713–723.

Headlee, A. J. W., & J. S. Joseph
1945. Permeability, porosity, oil, and water content of natural gas reservoirs. West Va. Geol. Surv. Bull. **8**.

Jager, E. H.
1942. Pre-Cretaceous topography of Western Edwards Plateau, Texas. Bull. Amer. Assoc. Petrol. Geol. **26**: 380–386.

Johnson, D. W.
1919. Shore processes and shoreline development: 75. New York.

Kay, M.
1945. Paleogeographic and palinspastic maps. Bull. Amer. Assoc. Petrol. Geol. **29**: 426–450.

Krumbein, W. C., & Esther Aberdeen
1937. The sediments of Barataria Bay. Jour. Sed. Pet. **7**: 3–17.

Krumbein, W. C., & L. T. Caldwell
1939. Areal variation of organic carbon content of Barataria Bay sediments. Bull. Amer. Assoc. Petrol. Geol. **23**: 582–594.

Krumbein, W. C., & J. S. Griffith
1938. Beach Environment in Little Sister Bay, Wisconsin. Bull. Geol. Soc. America **49**: 629–652.

Lee, W.
1943. The stratigraphy and structural development of the Forest City Basin in Kansas. Kansas Geol. Survey Bull. **51**.

Plummer, F. B., & E. C. Sargent
1931. Underground waters and subsurface temperatures of the Woodbine Sand in Northeast Texas. Univ. Texas Bull. **3138**.

Price, P. H., & A. J. W. Headlee
1942. Geochemistry of natural gas in Appalachian Province. Bull. Amer. Assoc. Petrol. Geol **26**: 19–35.

Pye, W. D.
1944. Petrology of Bethel Sandstone of South-central Illinois. Bull. Amer. Assoc. Petrol. Geol. **28**: 63–122.

Smithson, F.
1939. Statistical methods in sedimentary petrology. Geol. Mag. **76**: 207–309; 348–360; 417–427.

Torrey, P. D.
1934. Problems of Petroleum Geology. Amer. Assoc. Petrol. Geol.: 474.

Trainer, D. W.
1932. The Tully limestone of central New York. N. Y. State Museum Bull. **291**.

Weeks, W. B., & C. W. Alexander
1942. Schuler Field, Union County, Arkansas. Bull. Amer. Assoc. Petrol. Geol. **26**: 1467–1516.

SECTION OF PSYCHOLOGY

April 16, 1945

Doctor S. Bernard Wortis, Professor of Psychiatry, New York University College of Medicine; Director of Bellevue Psychiatric Hospital, New York, N. Y.: *Some Aspects of Military Neuropsychiatry.*

World War I played an important part in stimulating interest in psychiatry and psychiatric teaching. Much valuable experience was accumulated concerning the problems of (1) selection; (2) training; (3) detection of individuals vulnerable to the stresses of war and (4) morale-sustaining methods. It is estimated that, of our army of 3,500,000, approximately 70,000 were rejected for some mental aberration. Only those with glaring personality defects were weeded out. Even then, the cost in manpower, morale and money was high. However, too frequently, the psychoneurotic was looked upon as a malingerer or a weakling and was either seriously disciplined or rejected as being undesirable for military service.

Reports of the incidence of neuroses during World War I varied. The British Expeditionary Forces were said to have 34 neuroses per 1000 casualties; the Canadian Expeditionary Forces 24 per 1000, and, in the American Expeditionary Forces, neuroses were 9.5 per 1000 casualties. It is likely that our own American Expeditionary Force casualties were lower because our men were not as long in combat service as the British and Canadians.

It was also prevalent belief that few of the wounded developed psychoneuroses, and this seemed logically related to the fact that any disabling physical injury did the job of removing the soldier to a hospital away from the combat area, where he got sleep, food, rest, medical care and esteem that he had done his bit. Experience also indicated that other complex factors often were precipitating stresses that were related to the onset of neuropsychiatric disorders. Some of these were: anxiety; new responsibility; troublesome news from home, producing insecurity; physical injury or illness, resulting in deformity or amputation.

Morale of soldier patients on any ward was found to be of greatest importance in their recovery.

Neuropsychiatry in the military forces, during the current World War II, presented some new and many old problems. Experiences of the first World War, while serving as a guide, needed much adaptation

to the conditions of modern global warfare with its increased *tempo* and its new air, third dimensional, factor.

However, the underlying basic hazards to the soldier, today, are (1) separation from home and anxiety regarding family (this is especially important for the British, French and Russian soldier, who may have his family injured in the course of civilian bombing by enemy planes); (2) fatigue; (3) discipline; (4) monotony; (5) loss of liberty; (6) strange surroundings; (7) change of diet; (8) change in attitude toward authority; (9) change in the psychology of the peace-loving civilian to becoming a "killer" of the enemy; (10) fear of personal injury or deformity.

There are several points through which the soldier proceeds, where he may be screened for neuropsychiatric disability. These include the induction center, the recruit reception center, the replacement training center, in tactical units, in staging areas, in combat zones, convalescent hospitals, detention and rehabilitation centers.

Considering the small number of psychiatrists in the armed forces, they are doing a very effective job. The Army and Navy are also running training schools of neuropsychiatry to indoctrinate and educate medical officers in the understanding, diagnosis and treatment of neuropsychiatric problems.

There are three important factors that make for good morale in fighting men. These are (1) adequate war aim and purpose; (2) a conviction of one's competence and value, and (3) the feeling that one matters as an individual in one's group.

We now know that approximately over 1/3 of all army medical discharges are for neuropsychiatric illness. These, at present, total approximately 10,000 per month. In addition to the large number of clear neuropsychiatric problems, 50% of cardiovascular, 25% of gastrointestinal, and 10% of orthopedic problems, seen in military hospitals, are functional.

The following neuropsychiatric clinical responses are seen in military personnel:

(a) Strictly organic conditions (i.e., bodily injuries), in which emotional difficulties (wish to escape injury or combat) play an important factor in recovery.

(b) Borderline physical conditions in which the emotions play an important role; as, for example, in gastric ulcer, allergy, hypertension.

(c) Physical complaints, as an expression of emotional disorders.

(d) Predominantly psychological reactions, such as anxiety, fear, obsession, hysterical conversion, psychosis.

(e) Injuries to the nervous system with very little emotional complication.

Special consideration must be given to the understanding and treatment of our own repatriated prisoners of war. British experience indicates specific psychologic changes in such repatriated men. Two or three years as a prisoner results in readjustment difficulties. Observations of these men have shown that, after the first excitement and happiness of getting home has passed, a considerable number develop depressive apathy, bitterness, discontent, awkwardness. Most such men require at least six months of reintegration into the community. The repatriated prisoner is oversensitive to authority and we must remember that he has, for a long time (during his period in a prison camp), been an expert at evading and blocking authority. He needs to be treated with special understanding.

One hears much about "combat fatigue" or "operational fatigue"—a military euphemism. It is a distinct psychiatric entity which has four identifying criteria. These are (1) repeated anxiety nightmares, in which the whole or part of the traumatizing scene is relived; (2) increased startle reaction to noises and movements; (3) associated dysfunction of the autonomic nervous system, in the nature of tachycardia, increased sweating and gastrointestinal overactivity, and (4) recovery, following rest and psychotherapy.

Psychoneuroses are by far the commonest type of neuropsychiatric disturbance seen in fighting men. Anxiety states are the commonest reactions encountered. Character disorders with antisocial coloring, with hostility and bitterness, which may lead to alcoholism or over-resentment to authority, are also seen. In soldiers who develop such reactions under combat conditions, if neurotic character features predominate, it is often more effective to deal with these problems as neuroses rather than as psychopathic personalities.

Full blown psychoses are infrequent in soldiers. Some combat reactions are characterized by marked confusion, agitation, social disorientation, fear, and even hallucinatory experiences. Many such "encapsulated reactions" resemble schizophrenic reactions, but are really more allied to panic states, and those affected are found to recover quickly, when given adequate treatment and a safe environment.

Many psychiatric therapeutic methods have been used in this war,

some long, some short. Narcosis, shock therapy and procedures using pharmacologic aids have been found effective and often time-saving in the recovery process. I shall not consider these methods in detail in this abstract. However, group psychotherapeutic methods (especially when reinforced with occasional individual psychotherapeutic interviews, when indicated) have been found of much value. Essentially, group therapy is an educational procedure which gives psychological insight by regulated participation of the soldier. Because of the enormity of the job, it is obvious that individual treatment of large numbers of psychoneurotic soldiers is not possible. Often group participation helps the soldier by minimizing his personal problems and by partially transforming his personal problem into a group problem. He finds that others are like him.

In group therapy, one often uncovers various reactions and attitudes. Some soldiers become markedly tense, agitated and jittery, while relating their experiences with dramatic emphasis. Others are resentful and hostile and disinterested in their rehabilitation. They may project their guilt, fear, or inadequacy by blaming their superiors. Still others, accept their illness with seeming resignation and indicate that they have been nervously inclined all their lives. They accept their illness with relief and look forward to returning home. Very many are fundamentally stable, loyal, courageous men, who were exposed to more than it was reasonable to ask of them. The favorable response of soldiers to comparatively brief group psychotherapeutic help, warrants its wider adoption in both military and civilian psychiatry. Group therapy is best given to small groups of men.

Our knowledge of military neuropsychiatry has grown in this war and, undoubtedly, the experiences gained will advance our knowledge of man and how he reacts under stress. The first World War taught us much about the neuroses and changed our attitude toward the neurotic. The present World War has done much to further our appreciation of these problems and has indicated that medical, morale, and social problems are interrelated. We have learned that good morale is the best prophylactic—the best mental hygiene—to prevent psychoneurosis. Such morale is dependent upon the triad: knowledge of the justness of the cause one fights for, good military leadership, and good medical understanding and support for the soldier. Physicians now are better equipped to consider the total man and his functioning.

Doctor Edna Heidbreder, Professor of Psychology, Wellesley College, Wellesley, Mass.: *The Attainment of Concepts—A Psychological Interpretation.**

The purpose of this paper is to illustrate a hypothesis concerning human cognition by applying it to a particular set of experimental data.

The data were obtained from one of a series of experiments on the attainment of concepts by human adults. It should be emphasized at the outset that the data will be used illustratively—that they are not offered as sufficient in themselves to establish the hypothesis, but are presented as a useful means of exhibiting the hypothesis at work by showing how it engages with a set of experimentally obtained facts. For this purpose, it seems advisable to use a single set of data and to examine them in some detail.

THE HYPOTHESIS

The general hypothesis has been outlined in a previous publication.[3] That part of it which is relevant to the present discussion may be expressed in two statements:

1. The first is that, in human beings, as they are now constituted and conditioned, the perception of concrete objects is the dominant mode of cognitive reaction, i.e., the one most likely to occur in ordinary conditions of stimulation, and that other cognitive reactions are, in some sense, modifications of, or approximations to this dominant mode.

2. The second is that the attainment of concepts is an extension and refinement of the kind of reaction involved in the perception of concrete objects—that it carries the function of perception beyond the level possible to perception alone.

More specifically, the hypothesis maintains that human beings, in arriving at concepts and in using them, are performing a function very similar to the one they perform in perceiving concrete objects. In both cases, they respond in such a way that the organism is provided with units suitable to its characteristic modes of operating on the environment. In perceiving a concrete object—say, an apple—the organism is responding to the environment through its receptors in such a way that it is confronted with a unit to which it can react directly with its motor organs, a unit especially suited to the characteristically human mode of motor response, manipulation. In attaining a concept—say of the

* This address was delivered at the meeting of the Section of Psychology, March 19, 1945.

class, apples—the organism arrives at a unit not directly manipulable by the motor organs, but suited to another characteristically human mode of reaction, the symbolic. If this relation actually obtains between the perception of concrete objects and the attainment of concepts, it provides a psychological explanation for the liability of human beings to the reification of concepts—a liability which has aroused the interest and concern of many scholars; among them, operationists and logical positivists, students of semantics and of social psychology.

THE EXPERIMENT

The experiment from which the data were obtained was one in which 63 subjects, 29 men and 34 women, all university students, attained nine concepts each. The specific problem was that of determining the relative readiness with which concepts are attained when they deal with concrete objects, with spatial forms, and with numbers.

The method was a modified form of that devised by Hull.[4] In general, it was like that used in ordinary memory experiments. Each subject was required to learn the names—in this case, nonsense syllables—of a number of drawings presented to him singly and successively, in 16 series, each series presented at a mechanically controlled rate. The method differed from that of the ordinary memory experiment in that it was possible for the subject to discover, as he learned to name drawings in series after series, that though, in the various series, the same syllable was applied to many drawings, no two of which were alike, all those drawings possessed a common and distinguishing characteristic. The name, *Mulp,* for example, was always applied to a drawing of a tree—an oak, an elm, a maple, or a palm, but always to a drawing of a tree. This syllable was never applied to a drawing not picturing a tree. The word, *Fard,* was always applied to something circular, sometimes to a drawing of a concrete object like the face of a clock, a holly wreath, or a silver dollar; sometimes to drawings of circular designs not representing concrete objects. The concept of the circle was never pictured by a mere circular line; the drawing always contained something more than the bare circumference of a circle. The word, *Ling,* was always applied to a drawing of two exactly similar items—two concrete objects, or two figures or forms not picturing objects. In drawings representing numbers, not only the kind of item but the arrangement of the items was varied from series to series, so that neither

the kind of item nor the arrangement, as such, could serve as the basis of the concept. Nine concepts were studied in this experiment. Three were concepts of concrete objects—face, tree, and building; three were concepts of spatial forms—the circle, and two unconventionalized figures for which there are no names; and three were concepts of numbers—two, five and six.

The materials for the experiment consisted of 16 series of drawings, each series consisting of nine drawings, each drawing serving as an instance of one of the nine concepts. Within a single series, the drawings were arranged in a systematically random order which varied from series to series, so that any advantages or disadvantages that might arise from particular positions were equalized, and so that drawings could not be correctly named by learning a serial order or regular positions within the series.

It is not necessary to discuss the procedure in detail. The subject learned each of the 16 series up to the point of two successive, errorless repetitions. The usual records of prompts and correct responses were taken. By the time the subject had gone through the entire experiment, he had seen and had learned to name correctly 16 instances of each of the nine concepts.

At the end of the experiment, the subject took two brief, written tests. In one of these, he was presented with a list of the syllables, arranged in random order, and was asked to tell what each syllable meant—to indicate its meaning in any way he chose, not necessarily by a formal definition. In the other test, which was of the multiple choice variety, he was asked to underline that one of four presented items which best indicated the meaning of the accompanying syllable.

THE MEASURE: CONCEPT ATTAINED (C.A.)

Many measures are obtainable from the data of this experiment, e.g., the usual measures of learning. In this paper, however, a single measure will be considered, that which indicates the general outcome of the experiment, the order in which the concepts were attained.

Sooner or later—usually in the second or third series in this particular experiment—the subject, on the first presentation of a series, correctly and without being prompted, named some one drawing on its first appearance; i.e. he correctly named a drawing he had never before seen. As a rule, such responses were not followed in the immediately

subsequent series by consistently correct responses to other instances of the concept in question. But, eventually, in the case of each concept, the subject made a correct unprompted response to an instance on its first appearance, and followed this response by consistently correct responses throughout the rest of the experiment, correctly naming each instance of that concept, on its first appearance. When he did so, he is said to have attained the concept in question, and the ordinal number of the series in which this behavior *began* is used as the measure of the point at which the concept was attained. This measure, called Concept Attained, or C.A., constitutes the operational definition of the attainment of a concept as that term is used in this paper. It will be noticed that this measure is based, not on a single reaction, but on a stretch of consistently successful behavior. It is assumed that such behavior does not occur by chance, and it is inferred that, at the point at which the specified behavior appears, the subject has attained the concept with reference to which the experiment has been set up.

THE LOGIC OF THE EXPERIMENT

It is necessary, at this point, to comment on the underlying logic of the experiment. It will be noticed that no attempt is made to *observe* the psychological processes which presumably determine overt conceptual behavior, the behavior measured by C.A. Such psychological reactions are inferred from the observed and measured behavior, or rather from behavioral achievements in specified situations. "Achievement" is defined, in general, as better-than-chance success in a series of reactions. It is defined, more specifically, by the criterion stated in C.A. It is a basic assumption that, in the conditions of this experiment, behavior meeting this criterion would be extremely unlikely to occur *unless* the subject reacted with reference to the concepts about which the experimenter had designed the experiment. In this sense, the experimenter *introduces* concepts into the experimental situation much as an experimenter introduces mazes or pairs of stimuli into experiments on learning or sensory discrimination.

It will be noticed that the word "concept" here refers, and has referred all along, to a logical construct which can be used interpersonally, not to a psychological and individual event or formation. A concept is here defined as a logical construct, transferable from situation to situation and communicable from person to person. To refer

to the psychological processes, as such—the psychological reactions presumably involved in the attainment of concepts—such terms as "conception" and "conceptual reaction" are used. It is an underlying assumption that behavior indicative of the attainment of a concept is the outcome of reactions of the organism. But the nature of these reactions remains unspecified. These reactions—the conceptions, the conceptual reactions—are, in fact, initially defined as *whatever* reactions turn out, on suitable investigation, to be the cognitive determinants of the over conceptual behavior. Obviously, such terms are not descriptive of psychological processes. They are not even names of psychological processes of a specifiable kind. The nature of such processes can be known only through further research and inference—through research which more and more fully and precisely determines the conditions in which they occur, thus making possible increasingly definite statements concerning the inferred determinants of observed behavior.

RESULTS

The outcome of this particular experiment, in terms of the measure, C.A., is shown in TABLE 1. The means indicate that the concepts

TABLE 1

TABLE SHOWING MEANS OF CONCEPTS LISTED, ACCORDING TO THE MEASURE C.A., AND THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN MEANS AS INDICATED BY t.
(N = 63)

Concepts	Mean	t								
		Relk	Leth	Mulp	Fard	Pran	Stod	Ling	Mank	Dilt
Relk (face)	3.35		.59	3.47	4.27	4.25	4.97	5.17	5.95	6.48
Leth (building)	3.48	.59		2.09	3.27	3.83	4.28	4.84	5.80	6.48
Mulp (tree)	3.94	3.47	2.09		2.00	3.00	3.79	4.68	5.67	6.34
Fard (○)	4.46	4.27	3.27	2.00		1.31	2.09	3.43	5.12	5.88
Pran ( )	5.05	4.25	3.83	3.00	1.31		.31	1.98	4.52	5.68
Stod ( )	5.19	4.97	4.28	3.79	2.09	.31		2.07	4.70	5.53
Ling (2)	6.14	5.17	4.84	4.68	3.43	1.98	2.07		4.68	5.91
Mank (6)	8.76	5.95	5.80	5.67	5.12	4.52	4.70	4.68		2.86
Dilt (5)	10.22	6.48	6.48	6.34	5.88	5.68	5.53	5.91	2.86	

2.66 = sig. at 1% level of confidence
2.00 = sig. at 5% level of confidence

were attained in a definite order—concepts of concrete objects first, of spatial forms next, and of numbers last. This order is well established statistically. As a rule, concepts in one category are separated from those in other categories by differences statistically significant at high levels of confidence. The categories, however, seem not absolutely discontinuous. At both borderlines, there are indications of gradual rather than sudden transitions.

DISCUSSION

It is worth noting immediately that the obtained order is not positively correlated with some of the factors that might be considered advantageous.

(1) The order is not from the familiar to the unfamiliar. Two of the concepts, those of the unconventionalized spatial forms, were presumably attained for the first time during this experiment. Yet they appear in the middle of the list of concepts arranged in order of attainment, following the familiar concepts of concrete objects and of the circle, and followed by the familiar concepts of numbers. The three concepts of spatial forms, familiar and unfamiliar alike, are found together and in the middle of the list.

(2) The possession or lack of a conventional name does not seem a decisive factor. The two unfamiliar concepts were of course nameless, and they were both preceded and followed by concepts having conventional names.

(3) The order is not correlated with the relative difficulty of learning the nonsense syllables used as names. With one exception, there were no statistically significant differences in the rate at which the syllables were learned in the first series. The exception was *Ling*, which was learned significantly earlier than the others, but the concept, *Ling*, appears seventh in the list, arranged according to C.A., along with the other concepts of numbers. Besides, in control experiments, the names used in this experiment were assigned to other concepts in such a way that the names here used for concepts within a single category, were there distributed over the three categories. In the control experiments, the concepts were again attained in the order, objects—forms—numbers, regardless of the syllables used as names.

(4) The order cannot be correlated with any of the perceptual characteristics of the drawings as stimulus complexes, e.g., with such

characteristics as simplicity and "goodness of form." Since the experimental materials were not prepared and varied with respect to these factors, this point cannot receive adequate treatment. At best, it is difficult to define simplicity and goodness of form in drawings like those used in this experiment. But, by any definition, visual circles would be considered simpler as spatial forms than pictured concrete objects. Yet the concept of the circle was attained not first, but fourth, following the concepts of concrete objects. Visual circles, too, show goodness of form to a high degree, since they are characterized by unity, compactness, symmetry and good continuation.

It may be said, too, and very definitely, that, as stimulus complexes, the drawings of spatial forms had more uniformity of visual pattern from instance to instance, than did the drawings for any other concept or set of concepts. This, too, was evidently not a decisive factor.

This, of course, constitutes only a partial list of possible determinants, none of which has been treated exhaustively. It is possible, too, that the explanation lies in some combination of the factors considered, or in some combination of these with factors not yet taken into account. One concept, in fact, seems to be favored to a high degree by most of the factors already mentioned. This is the concept of the circle, which has whatever advantages go with familiarity, a conventional name, maximal uniformity of pattern from instance to instance, and goodness of form to a high degree. Such considerations raise the question: Why was the concept of the circle not attained first? Just what determines the earlier attainment of concepts of concrete objects?

Questions like these suggest that the obtained order is not so obvious a fact as it seems at first glance. Yet, it is well to notice that the order *does* give an immediate impression of obviousness. The order, it may be said, goes from the concrete to the abstract, and it is common knowledge that concepts are harder to attain in proportion to their abstractness. But all concepts are abstract. What is meant by degrees of abstractness? Perhaps, in the limited set of concepts here considered, a concept may be called more or less abstract according to the relative degree of concreteness or abstractness that characterizes whatever is common to all its instances. But in what sense, if any, is a perceived object—say, a seen phonograph record—more concrete than its visible circular form? And in what sense, if any, is the duality of two phono-

graph records, seen side by side, less concrete than the visible circular form of either of them?

Psychologists do not agree among themselves on what constitutes maximal concreteness, though they are likely to equate the concrete with the "given." One view is that of the classical experimental psychologists who find the concrete and the given in sensory and affective elements. Another is that of the gestaltists who find it in experience already "formed." According to neither of these, it will be noticed, is maximal concreteness—in the sense of maximal givenness—found in what, in common, everyday speech and in this experiment, is called a concrete object. Under scrutiny, the obviousness of the order disappears.

A passage in Carnap's *Unity of Science*[2] pointedly illustrates the lack of agreement among psychologists on what is "given" as opposed to "derived." The passage occurs in the discussion of the important topic of protocol statements: "the statements belonging to the basic protocol or direct record of a scientist's (say a physicist's or psychologist's) experience," . . . "the statements needing no justification and serving as foundation for all the remaining statements of science." Significantly, the author finds it impossible, in the present stage of research, to characterize the protocol language precisely. Instead, he indicates the meaning of the term by sketching "some of the views as to the form of protocol statements held at the present day by various schools of thought."

According to one of these, protocol statements are of the same form, as: "here, now, blue; there, red." This point of view is similar to that just attributed to the classical experimental psychologists. Another is that of Gestalt Psychology, according to which protocol statements are of the form, "red circle, now," or are statements about "entire sensory fields, e.g. the visual field as a unity," or about "the total experience during an instant as a unity still undivided into separate sense-regions." Carnap also presents a third possibility. He suggests that protocol statements might take the same kind of form as "a red cube is on the table." This view, he says, "is not often held today; it is however more plausible than it appears and deserves more detailed investigation."

One way of stating the problem raised by the data now under consideration is: Is it, for any reason, *psychologically* more convenient for

human beings to begin with such statements as, "a red cube is on the table."?

The general direction of the answer has already been indicated in the statement that the dominant cognitive response in human beings is the perception of concrete objects. But, obviously, it is necessary to explain more fully what is meant by a concrete object and by perceiving a concrete object.

A concrete object is taken, in the first place, as naively and unreflectively as possible—as typically a visible, tangible, manipulable body in the external world. The perception of objects is, itself, initially defined with reference to such objects, or rather, with reference to the subject's behavior with and toward them. It is defined as consisting of *whatever* processes turn out upon suitable investigation to be inferable as cognitive determinants of observed behavior related to concrete objects in specified ways.

In selecting the perception of concrete objects as one of its main points of reference, the hypothesis chooses an activity which is neither simple nor primitive. Perceiving an object is admittedly a highly complex affair. All psychologists would agree that it includes sensory reactions, but that sensory reactions are not enough; that a perceived concrete object can not be exhaustively described in terms of sensory content alone. They would agree, too, though their interpretations would differ, that in a perceived object, the various sensory contents are in some way integrated, the sensory stuff somehow delimited, stabilized, and formed. They would also agree, even the staunchest nativists among them, that the perception of a concrete object is commonly determined—and strongly determined—by learning and experience, including, of course, acculturation. They would again agree that what is present to the senses does not stand alone; that it must be supplemented in some way, if the organism is to perceive a concrete object. In the present hypothesis, it is regarded as an essential fact about a perceived object that what is present to the senses is reacted to as a sign of more than the sensibly present, of the whole object, not excluding its functional significance; that the visible rounded redness of an apple is, to the percipient, a sign of its other side, of its inside, and also of its edibility. Thus, the present hypothesis not only accepts but emphasizes the complexity of the perception of a concrete object, and it also maintains that such complex responses are now dominant in human cognition; that, instead of making the barest and simplest responses at

their disposal, human beings respond in a manner, more complex and voluminous, in which the simpler and barer responses are caught up as constituents.

It should also be noted that the perception of objects is biologically far from primitive. This, indeed, is implied in the statement that it is the cognitive correlate of manipulation. It has long been recognized that motor organs precede sense organs in evolutionary developments and that manipulatory behavior occurs late in the phylogenetic scale. It is interesting, in this connection, that manipulation reaches its highest proficiency in man; as another highly developed motor activity, locomotion, does not. Furthermore, it is possible to claim as structural correlates for the perception of objects in man, not only prehensile hands, but the physiological basis for stereoscopic vision, and also for encephalized and corticalized behavior, and, thus, for the integration of sensory activities with each other and with motor behavior. To call attention to these biological facts is not, of course, to say that perceiving an object is independent of learning and experience. Perceiving objects is probably among the most practiced of human activities; practiced, furthermore, in the circumstances probably most favorable to learning, in the thick of the urgent needs and actions, the immediate satisfactions and disappointments of daily life. The point here made is that the biological facts reveal the perception of objects as by no means primitive, and that the hypothesis maintains that this far from primitive response is now the dominant mode of cognition in man.

The hypothesis further maintains that, if the perception of objects is regarded as dominant in human cognition, other cognitive activities, specifically conceptual reactions, become intelligible as modifications of this mode of response. In the present hypothesis, conceptual activities are treated as refinements and extensions of perceptual activities, performing, beyond the level of sensory perception, essentially the same function performed at that level by the perception of concrete objects, providing the organism with units suitable to its modes of reaction. In perceiving a concrete object, an organism responds selectively to functionally significant aspects of the environment, responding directly to sensory stimulation, and reacting in such a way that, in the very process of receiving stimulation, a delimited, integrated unit is marked out and made salient, suitable to the guidance and control of motor manipulation. In attaining a concept, an organism again responds selectively, but now indirectly; now by abstracting whatever is relevant to the

enterprise in progress.. Again it integrates; now by generalizing, in some sense bringing together what is common to many separate, particular occasions. Again it delimits; now by a definition more or less precise. And again the outcome is a functionally significant unit; now suited to another mode of operation of which the organism is capable, reaction involving the use of symbols.

When the hypothesis, so interpreted, is applied to the experimental data, these fall into line rather readily. The functional core of the conceptual activity may now be located in the selective reaction which isolates and delimits whatever is common to the instances of the concept in question, thus performing the relevant abstraction. It may now be said that the order in which the concepts were attained corresponds directly to the degree to which this selective reaction requires departure from the dominant cognitive response. Those concepts were attained first, in which the relevant abstraction can be determined by reacting to the drawings as pictured objects or things—trees, faces or buildings. Next, came those in which the abstraction can be determined by reacting to the drawings as visible spatial forms—by reacting, in each drawing, to something less than a thing but not altogether un-thing-like. And next came the concepts in which the relevant abstraction can be determined by reacting, not to pictured things, not to continuous units of any sort, but to collections, or rather to facts about (numerical quantities of) collections—a performance which seems more remote from perceiving a concrete object than is a response to a visual form. But perhaps this account seems somewhat facile. So stated, it leaves the problem on an impressionistic level. To make a more precise statement, it is necessary to tell more explicitly and in still more detail (1) what is meant by a perceived thing and (2) what is meant by degrees of departure from perceiving a concrete object.

And so, once again, what is a perceived thing? An approach to this problem is suggested by the fact that concepts of concrete objects were regularly attained earlier than that of the circle, though the concept of the circle is favored by more of the factors, so far considered advantageous, than is any other concept in the experiment. Can any advantage be attributed to a perceived thing and not to a visual circular form, that might account for the readier attainment of concepts of concrete objects?

One answer immediately suggests itself: manipulability, relevance to direct motor reaction. A circular form, even when visibly and tac-

tually present, is not subject to manipulation. One can, of course, manipulate a circular object like a wheel. One can even construct a circular form, an actual visible circular track of chalk on a blackboard, and one can alter the visible form by erasing parts and redrawing them. But what one manipulates is always a concrete object, a wheel or a piece of chalk or an eraser. Even if one describes a circle with a finger in the empty air, it is a thing one manipulates, an arm terminating in a finger. It is interesting that a visible circular form is more completely exhibitable, more completely demonstrable to sensory perception by a motor gesture, than is a visible concrete object. The former can be more fully and adequately indicated by a motor reaction, e.g., by running a finger around the edge of a coin or a dinner plate. The object itself, the coin or the dinner plate, can only be pointed to; it cannot be fully exhibited. Yet, a circular form, as such, no matter how perceptible, cannot be picked up and moved from place to place. There is some sense in saying that it just misses being manipulable. It is just beyond the finger tips, but completely beyond them; hence, it completely escapes motor manipulation, though it is as completely perceptible as anything can be. This point is highly significant in relation to a set of data in which the concept of circularity, with all its advantages, was attained significantly later, though only a little later, than concepts of concrete objects.

From manipulability, it is only a step to the dynamic properties discussed by Koffka[5] in his treatment of thing-character. It will be remembered that Koffka lists three constituents of thing-character: shaped boundedness, constancy, and dynamic properties; and that he makes much of the point that there is no sharp line of demarcation between things and not-things.

This description of thing-character is distinctly relevant to the present hypothesis, but here the three constituents are regarded as not on exactly the same basis from the standpoint of the psychological processes involved. Here, only two of these constituents—constancy and shaped boundedness—are treated as present to the senses. And these are here treated as *signs* of properties not present to sensory perception—not only of actually unperceived but potentially perceptible properties (like the color of the other side of an apple) but also, and very importantly, as signs of dynamic properties which cannot themselves be apprehended by the senses and must, somehow, be constructed. Furthermore, the dynamic properties of thing-character are regarded as functionally central to the perceptible constituents of thing-charac-

ter. It is regarded as part of the function of shaped boundedness and constancy to signify—to locate and signal the presence of—dynamic properties. To perceive an object, then, is to respond selectively and through the direct action of the receptors, to a dynamically effective and functionally significant portion of the environment, and to respond in such a way that the organism is confronted with an integrated unit, relevant to its needs and adapted to its characteristic modes of motor reaction. Such units are the perceived concrete objects of the present hypothesis.

To define the perception of concrete objects in this way and to regard this mode of reaction as cognitively dominant, is to treat human cognition as dynamic at the core of its organization. It is to say that the very way the world looks—and feels and smells and tastes and sounds—is determined by the needs of the organism and by its capacities for satisfying those needs. It is to make a place, in the very way human beings apprehend the environment, for learning and experience; including, of course, the operation of social and cultural factors. It is, also, to claim that, in the response in which human cognition is centered, in the very way in which a human being takes hold of the environment cognitively, constructive activity of some sort is essentially involved. Bridgman[1] illustrates this point by calling attention to the inside of an object, which can never be exhibited; since, if the object is cut in two, the inside becomes, by definition, the outside. But the point emphasized in the present hypothesis is that the dynamic properties of an object are in some way constructed; and that these, and, hence, the functional significance of an object, are psychologically apprehended as inhering in the object. Thus, a perceived thing marks the point at which the human organism engages directly with the environment in its cognitively dominant response; the point in which the dynamic properties of the environment and the needs and reactive capacities of the organism converge.

This long account of a perceived thing, it will be remembered, arose from the question: What advantage can be attributed to perceived things and not to visible circular forms? The first impressionistic answer, manipulability, may now be translated into a more explicit statement. The manipulability which a circular form lacks and which an object characteristically possesses, is related to the fact that a circle has only two of the properties of thing-character, and the object all three. Apparently, the perceptual prominence of an object may be

ascribed to the fact that it possesses full thing-character, including dynamic properties. Dominance in cognitive reaction seems to be correlated, not with maximal openness to inspection, not with maximal "givenness" in perceptual experience, but with maximal relevance to action, specifically to manipulation, that kind of motor reaction which human beings characteristically employ.

If relevance to action determines dominance, in conceptual as in perceptual reactions, those conceptual reactions should be most readily aroused which are closest in function to the perception of concrete objects. And if conceptual reactions are, as this hypothesis maintains, modifications and extensions of the perception of objects, the primary conceptual function should be that of supplementing perceptual organization. It should be precisely that of carrying a stage farther than is possible for perception alone, the organization of the perceived environment in a manner suitable to motor reaction. The first step in this direction would be the attainment of concepts of concrete objects—of *things*. For, to react to things conceptually, is to react not merely to *this* thing, but to this *kind* of thing; not merely to see *this* apple, but to see this *as an* apple. To react in this manner is not merely to give prominence to particular objects in a world perceptually organized for immediate action; it is, in the very act of perceiving such objects in such a world, to apprehend them as *kinds* of things, which call for *kinds* of action. It is to utilize the opportunities provided by the distance receptors for delay in reaction and for preparatory adjustment. It is also, and very importantly, to utilize the products of learning, including the learned expectations which, with varying degrees of complexity and remoteness, control motor reaction.

When these considerations are brought to bear upon the experimental data, the hypothesis becomes applicable in a less impressionistic manner. Degrees of departure from the perception of objects may now be correlated with degrees of thing-character attributable to those aspects of, or facts about, perceptual situations which the subject's reaction must take into account, if the relevant abstraction is to be made; and degrees of thing-character may, in turn, be correlated with degrees of relevance to motor reaction. Thus, the early attainment of concepts of concrete objects may be explained by the fact that, as compared with the other concepts in this experiment, their attainment requires less departure from the perception of objects, and that, in function, they are relevant to the organization of the perceptual field for motor reac-

tion. The order in which the other concepts were attained may be similarly explained.

First, why were concepts of spatial forms attained later than concepts of concrete objects? Chiefly, because the instances to which they apply have only partial thing-character. To respond to a visible form is to respond to *less* than a thing, to make a response not only less dominant perceptually than that of perceiving a concrete object, but less immediately relevant to motor manipulation. It is especially interesting that, of all the concepts included in this study, instances of the concepts of spatial forms were most completely "given" in the perceptual materials; whereas, any reaction to a pictured object was necessarily made—as is any reaction to an actual object—to a stimulus complex which does not fully exhibit the object to sensory perception. Again, it is worth noting that complete perceptual "givenness" seems less potent in determining readiness of reaction than does close functional relevance to manipulatory behavior.

It should also be noted that, in making the transition from one category to another, the reaction is evidently not a simple, abrupt, all-or-none affair. The familiar concept of the circle was attained only a little later than the concepts of concrete objects, and the unconventionalized, unfamiliar concepts *pran* and *stod* were not attained significantly earlier than the earliest concept of number. Their attainment later than that of the circle may have been influenced by the fact that they had to be constituted anew, and perhaps also by the fact that they lacked goodness of form and conventional status. Yet, these facts hardly account for their attainment earlier than the concept *two*. The striking fact is that the three concepts of spatial forms—familiar and unfamiliar, conventionalized and unconventionalized, "good" and "not-good"—were attained together, as if held together by a common determinant, their relevance to perceptual situations which possess partial thing-character.

The next question is: Why were concepts of numbers attained later than concepts of spatial forms? A special significance attaches to this question because all the concepts of number were well known to the subjects, whereas two of the concepts of spatial forms, as has just been noted, had to be constituted anew. Again, the answer can be given in terms of thing-character. Concepts of spatial forms apply to situations having partial thing-character and concepts of numbers are applicable only to aspects of, or facts about, perceived situations—to aspects

and facts possessing none of the properties of thing-character. Visible plurality, to be sure, may be perceptually apprehended; but preceptually apprehended plurality lacks the perceptual characteristics of thing-character. Plurality, in so far as it is perceptible, is supra-local. It possesses neither constancy nor shaped-boundedness.

Such perceptual plurality is not a sufficient basis for the identification of instances of *five* and *six*. There is evidence that subjects did, in fact, immediately react to plurality in instances of *five* and *six*, but not to plurality, as such; not to plurality apart from other aspects of the situation. They spoke of many small objects or figures, or of arrangements of many small objects or figures, but, on the basis of such reactions, the instances of *five* and *six* were thoroughly confused. To make a precise determination of numerical quantity requires going outside the perceptual situation itself—establishing a one-to-one correspondence between the items perceived and something not itself included in the situation to which the numerical determination applies. Such a procedure, in short, requires the use of symbols—perhaps the words used in counting, perhaps something as concrete as the fingers of the hands—but something apart from the situation to which the concept applies, something used as a *symbol*, not merely reacted to as a *sign*. In such reactions, the constructive component becomes more prominent because less merged with what is present to the senses; the concept is applicable to something less thing-like than perceptible objects less thing-like, even, than visual forms; hence it is more remotely related to motor action.

The concept, *two*, is especially interesting in this connection. Experiments on the span of visual apprehension leave no reasonable doubt that duality can be apprehended perceptually, i.e., that it can be identified without counting. Yet the concept, *two*, was attained later than the concepts of the unconventionalized forms, which were unfamiliar, which had to be constituted anew, and which were not particularly "good." Evidently, the partial thing-character of the instances of *stod* and *pran*, more than counterbalanced the factors favoring the concept, *two*. *Two* seems indeed a crucial case. It not only represents plurality reduced to a minimum, but its attainment is presumably favored by familiarity, conventional status, and the fact that duality can be apprehended perceptually. Perceptual duality, however, falls short of even partial thing-character; and the concept, *two*, is attained later than the concepts of spatial forms which seem to be favored only by the

partial thing-character of their instances, or at least by little else.

The successive steps by which the concepts of numbers were attained are indicated rather clearly in the series-by-series records of the subjects' overt performances, especially when these records are supplemented by the definitions offered by the subjects. It will be remembered that plurality was reacted to from the first, but that it was not made focal immediately. During the first series in the experiment, when the subjects were learning to identify the drawings by name, they seemed to refer the names to pictured concrete objects whenever possible, even reading objects into drawings in which no pictured objects were included. For example, the concept *mank* or *six* was represented in the first series by a drawing in which the six items were simply wavy lines. One definition of *mank* contains the statement, "First, I thought of them as *banks* or *manks* of clouds." Here, objects are mentioned and they are mentioned in the plural. Most of the subjects next passed through a stage of reacting to various aspects of the situation not themselves quantitative, and yet not unrelated to plurality. They tried to identify drawings by particular spatial arrangements of the separate items, or by supra-local spatial characteristics like symmetry or asymmetry, or by vague impressions that the number of items was odd or even, though the number was not specified. Eventually, most of the subjects arrived at the exact statable numerical determination, but some, though they gave consistently correct overt reactions, were unable to give adequate definitions. One definition indicates the course of events with exceptional clarity and completeness as well as brevity. It refers to the concept *dilt* or *five,* which was represented in the first series by a drawing of five small drinking glasses. The definition reads: "Dilt. Five. I remembered it at first by thinking of dealing out drinks (dealt—dilt). Then, I thought the design was always tilted (dilted) and then I noticed that there were always five of them."

Such sequences illustrate what is meant by saying that the order of readiness among conceptual reactions is correlated with the degree of their relevance to direct action, or rather with the closeness of their cooperation with perception in its function of organizing the environment in a manner suitable to the reactive capacities of the human organism. Those concepts were attained first, which are applicable to manipulable things—to perceptible concrete objects having full thing-character. Next, came those applicable to situations which fall short of manipulability—to visual spatial forms having partial thing-character.

These, however, are traceable with eyes or fingers, and can to that extent directly control motor reactions. And last to be attained, were the concepts of numbers—concepts which are applicable to perceptual situations only through the use of symbols; i.e. applicable to aspects or facts inaccessible to direct motor reaction.

CLOSING COMMENT

It would be inappropriate to close this discussion without repeating that the experimental data have been used merely illustratively. They are, of course, insufficient to establish the hypothesis employed in interpreting them. They have been used partly to show how that hypothesis may be applied to a particular set of experimental facts; partly, also, to suggest that it may be useful if applied more widely. The hypothesis especially emphasizes the treatment of cognition as an activity closely related to the going enterprises of the organism. It has been used in this paper to show that degrees of readiness in a particular set of conceptual reactions may be explained by assuming (1) that the perception of concrete objects, the cognitive correlate of manipulatory behavior, is the dominant mode of cognition in human beings; and (2) that conceptual reactions, the cognitive correlates of symbolic behavior, are functionally extensions of the perception of objects; and that the readiness of their occurrence is correlated with the closeness of their relevance to the perceptual function of organizing the environment in a manner suitable to the reactive capacities of human organisms.

1. **Bridgman, P. W.**
1937. The logic of modern physics: 42–48; 53–54. Macmillan, New York.

2. **Carnap, R.**
1934. The unity of science (tr. by M. Black): Kegan Paul, Trench, Trubner and Co. London.

3. **Heidbreder, E.**
1945. Toward a dynamic psychology of cognition. Psychol. Rev. **52**: 1–22.

4. **Hull, C. L.**
1920. Quantitative aspects of the evolution of concepts: an experimental study. Psychol. Monogr. 28 (123).

5. **Koffka, K.**
1935. Principles of gestalt psychology. Harcourt, Brace and Co., New York.

SECTION OF ANTHROPOLOGY

APRIL 23, 1945

DOCTOR HARRY L. SHAPIRO, Curator of Anthropology, American Museum of Natural History, New York, N. Y.: *The Biology of the Population of the United States.*

The study of human populations represents one of those areas where diverse disciplines may, and, in fact, do cooperate with mutual benefit. Since mankind functions biologically, reproduces, migrates, expands, declines or dies in some kind of ecological and cultural context that may affect any or all these phenomena, it would be folly to ignore the total picture as far as it is possible to know it. Nevertheless, by virtue of training, competence or interest, we may, perhaps, be permitted to approach the subject with rather specialized emphasis. This, I have chosen to do. But, in addition, the severe limitation of the time available to me in the face of a field as complex as this imposes the necessity of an even more rigid selection of topics than my inclination might otherwise suggest. I have elected, therefore, to deal only with the population of the United States and, within those limits, to discuss only a few of the biological aspects of the subject.

As a background to my comments on certain biological processes that are now affecting or are likely to influence the people of this country, I offer the following general characterization of the population of the United States. I venture to do this in the belief that not all the traits of a situation must be enumerated and described in detail to paint a recognizable and significant picture. The features I have stressed as characteristic of this population are admittedly only a fraction of those that might be listed, but my purpose is to provide a frame for the present discussion.

(1) Although the population of the United States is frequently referred to as young and immature, this is not a strictly accurate appraisal. In the sense that maturity of a population approximates a condition of relative stability, the population of the United States, in some respects, has advanced with great rapidity to achieve a status that is fully mature and relatively stable, although, in other directions, mentioned below, it has remained immature.

(2) In point of numbers and growth, the United States, paradoxically, one of the youngest countries, is one of the most mature. In

slightly over 300 years, we have increased from little more than zero to 132 millions. This expansion of population alone does not indicate the attainment of a stabilized population, but the fact that the rate of growth has declined rapidly, in recent years, to the point where further increase is likely to be very slow, indeed, is highly significant. The prospects indicate a relatively stable population of between 150 and 160 millions attained by the next generation. The estimates of the size and growth rate of the population, in the immediate future, are fairly reliable, since they are based on existing conditions that cannot be materially altered except by a national devastation of unprecedented magnitude. As far as numbers are concerned, what we are determines what we will be, and quick adjustments to fit economic or cultural changes are impossible. Economic depression may render 15,000,000 people superfluous in the scheme of things, or a war may create a demand for as many millions that we do not have, but populations cannot fit these fluctuations rapidly or easily. We must, therefore, adjust our social policies to these trends, wherever they may lead, or else adopt a population policy consonant with our social objectives and the realities of world affairs.

(3) The population of the United States is, today, one of the urbanized, which by implication usually means industrialized, nations of the world. Not only are the rural and farm populations decreasing relatively, they are losing numbers by absolute count as well. In 1790, this country had 94.9% of the population living and working in rural districts and 5.1% living in cities which were but little removed from the rural. Today, only 43.5% live in rural districts, while 56.5% are found in urban areas. This shift of balance is specially characteristic of Western European countries where industrialization has advanced most. Over 60% of Germany's population was urban and about 80% of England's. France, on the contrary, has remained more rural. In the United States, the movement, apparently, has not yet reached the degree found elsewhere, but in its rapidity it is exceptional.

Aside from the social and economic consequences of such a shift from rural to urban living, the biological results are likely to be significant, judging by present trends and current information. The movement toward urban centers in this country, at least, seems to draw off from the rural areas the best elements (along with others). Urban dwellers show a reproduction rate below the replacement level. This

conjunction, in the opinion of most students, leads to a reduction in the best strains in the population.

Urbanization has another effect of vast consequence. It permits the segregation of large numbers in nationality—minority groups,—undigested masses that for one reason or another elect or are impelled to maintain group solidarities, and, thereby, gives rise to the political consequence of the "Irish vote," the "Italian vote" and other minority voting groups. The retention of such group identities slows down the process of amalgamation and preserves cleavages along lines of national origin.

(4) Although no populations, with the possible exception of such minuscule realms as Luxembourg and Monaco, live in a completely uniform environment within their national boundaries, there are, on the contrary, few that embrace such a heterogeneous environment as that of our country. Russia, China, India, Brazil, Canada, and but few others compete with the United States in variety of climate, soil, topography and other such natural phenomena. These physical aspects of the United States are not, of course, characteristic *per se* of the population, but they do have a profound influence on the developing patterns of our population and, therefore, cannot be ignored.

(5) Culturally, on the other hand, the United States is relatively homogeneous. I am inclined to regard this as a trait of immaturity, the result of the celerity with which this country was settled at a time when communications were highly developed. Much of the United States has a history of settlement less than a century old. And if we go back 200 years, we find ourselves in an era when our population was almost entirely confined to the Atlantic coastal belt. It is admittedly difficult to measure by any objective standard varying degrees of regional differentiation, but if we compare such nations as France, Great Britain, Germany and Italy with our own, one is impressed by the highly developed local cultures in European countries and by their relative weakness here. Culture differentiation in this country, when it occurs, is more apt to be vertical than geographically horizontal in distribution. Although it would be inaccurate to assert that our cultural homogeneity is responsible for the high degree of population mobility in this country, it seems very probable it does facilitate movement of people and permits a readier miscegenation, except when racial and nationality attitudes intervene.

(6) Ecologically, the population of the United States has far to go

before reaching the stable balance with nature that some nations have achieved. The failure to conserve our national resources has resulted in deforestation, soil erosion, dust bowls and other calamitous events, which indicate the extent to which we have been out of step with nature. Our pioneers have been bitterly criticized for the havoc they have wrought, but it would be more reasonable to blame circumstances. Whatever the cause, the effects of maladjustments of this kind tend to keep the population mobile. Blighted areas become regions of emigration whose streams of displaced families become active agents in various dynamic population processes.

(7) Biologically, our population is heterogeneous. At least 10% is Negro and Negro-white. Another 1% or more includes Mexicans, Indians and various Orientals. The remainder, classed as white, embraces every nationality in Europe and a few from Asia. Many of these white groups, and all of the non-whites, form considerable bodies of people who find themselves segregated into a minority status. Such racial, national or religious groups may have no official political recognition, but they are realities in the social and economic processes of the country, and must be taken into account in the biological development of the nation.

In the time left me, I should like to elaborate a little on several points suggested by this summary of selected diagnostic features. I have mentioned that the population of the United States is rapidly reaching a climax or a plateau where, for the first time in its history, it will no longer be an expanding one. Presumably, if present trends in vital statistics are also stabilized, the size of our population will fluctuate around its climax number. The reason for this is the radical change in the age structure of our population. In 1790, 51% of the total population were 16 years of age or over, while in 1930 roughly 69% fell in this age group. The projected population of 1980 will probably have something close to 80% in the same age group. The median age in 1790 was therefore slightly over 16 years. In 1940, it was 29.0 years and, in 1980, in all probability, it will be in the middle or upper 30's. If we take those from 60 years of age and over, this proportion to the total population will have increased from short of 5% in 1850, to about 20% in the projected population of 1980. If the birth-death rates become stabilized in the next generation, this overbalanced age structure will slowly correct itself, but not until such a population has undergone a considerable decline.

We are, in any event, faced for a long time to come with an age structure heavily weighted at its apex. This, of course, means that there are fewer in the younger groups to replace the older strata and that the younger age groups, with their normal losses, will be unable to replace the reproducing age groups. Those who have been urging wholesale birth control are likely, in the next generation, to be out of favor, once the actual census returns begin to reveal the state of affairs now existing. The biological consequences may be charted statistically with some assurance, but the equally real if subtler effects on social and economic concerns are less susceptible to exact measurement. Nevertheless, this profound change in age distributions is bound to have wide ramifications in a number of directions. Already, the social and economic responsibility for the rapidly growing class of superannuated is beginning to be felt. The relative decline in the numbers available for the productive work of society may be met by technological developments, but should war come again, there would be no substitutes for an ample source of potential soldiers.

Disease and sickness patterns also exhibit a marked trend in the direction characteristic of populations with large concentrations in the upper age groups. Deaths from degenerative diseases and protracted illnesses due to chronic and incurable ailments are on the rise, and medicine has begun to develop, in response to this, the special study of the aged, geriatrics. Hospitals designed to care for prolonged and incapacitating invalidism among the elderly are being built with increased frequency, as the need becomes more pressing.

Less tangible, perhaps, than these institutional results, is the psychological pressure of an aging population on the social and political policies of the country. Although we have no critical studies of such a relationship, experience alone would suggest that the increasing median age of the population of the United States and Western Europe has not only contributed directly to certain social movements, but has also aided in more subtle ways to create the "climate of opinion" in which we live.

The heterogeneity of the population of the United States, which I mentioned previously, is only biological in part—in the sense that distinct genetic and morphological differentiation is possible. Some of our minority groups are more the expression of historical and psychological attitudes than they are of biological differences. The United States, settled in a period of self-consciousness, has evolved a well de-

veloped hierarchy of values based on the historical accidents of time of settlement, culture, religion, and nationalism. Whatever the cause, the lines of cleavage are often very real and persistent. The danger lies in their widening beyond the point where the accretions of time may close their gaps. The minorities problems of Central Europe are an indication of how festering they may become and how persistent in spite of negligible biological distinctions.

There are two ways by which these minority groups as tight entities may be dissolved. One is by the development of a social tolerance that places no disability upon the members of such minorities. The other is by miscegenation. Neither is, of course, exclusive of the other.

The development of social liberality is, in no sense, a biological problem, except in so far as social prejudice may claim a biological rationale. Miscegenation, however, does raise valid biological questions where genetic differences are real.

Recognizing that some degree of miscegenation is taking place all the time in our population, it is still possible to assert (1) that it is not proceeding rapidly enough in certain areas to break down inbreeding minority groupings, and (2) that certain patterns of amalgamation are characteristic and relatively frequent. Religion, national origin, race, language, social status, prestige and various other factors continue, in diverse ways, to establish these lines of miscegenation.

As far as the biological consequences of miscegenation are concerned, little attention has been paid to crossing between the various white groups. The general public, on the whole, has been unconcerned with such aspects of the situation. It is quite the reverse with mixture teween Negroes and whites. The very size of the mulatto population (not wholly the result of original crossing), is an index, if one were needed, that miscegenation has gone on at a considerable rate, but most of it dates back to slavery days when social conditions were different. Unlike miscegenation among whites, the union of Negro and white has not led to amalgamation. Mulattoes are classed as Negro and are burdened with the disabilities suffered by the Negroes. Only when "crossing the line" occurs does the so-called Negro attain amalgamation with the whites, but genetically at the cost of losing virtually all the "Negro" genes.

Americans, representing a mixture of various European strains, and exposed to environmental conditions unlike those in the Old World, are said to be evolving a new physical type. The evidence of a stead-

ily increasing size is incontrovertible. Our national average for stature surpasses the means of the European nations from which most of our population is derived. This increase, while a general phenomenon, is most marked in groups favored economically, with the result that college students yield averages well above the general level. Along with the size increase, have gone certain alterations in bodily proportion that follow the growth gradient and are not new developments.

Although our population has displayed this tendency to a greater degree than any other, the trend is not unique with us. Most people in Europe, for whom we have data, show the same increase. Japanese figures follow a similar trend.

Professor Mills, on the basis of a recent study on American college students, has come to the conclusion that the tendency to increasing size has reached its limit. It will be interesting, as new data becomes available, to check this finding and to determine if European populations are also following the same pattern.

The causes of this expansion of bodily size are obscure. Nutrition, undoubtedly, plays some part in producing the observed effect, but it does not seem to be the only factor involved.

SECTION OF BIOLOGY

April 13 and 14, 1945

Conference on *"Lymph."*

The Section of Biology held a Conference on "Lymph," as the sixth in the series for the Academic Year 1944–1945. Doctor Philip D. McMaster, Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"The Topography and Functional Activity of the Blood Capillary Bed with Special Reference to Visceral Tissue," by Robert Chambers, Washington Square College of Arts and Sciences, New York University, New York, N. Y.

"Blood Circulation in the Subcutaneous Tissue of the Living Bat's Wing," by Paul Nicoll, Department of Physiology, Indiana University, Bloomington, Indiana.

"Factors Affecting Capillary Permeability and the Composition of Capillary Filtrate," by Eugene M. Landis, Department of Physiology, Harvard University Medical School, Boston, Massachusetts.

"Intercellular Substance in Relation to Tissue Growth," by Eliot R. Clark, Department of Anatomy, University of Pennsylvania, Philadelphia, Pennsylvania.

"Conditions in the Skin Influencing Interstitial Fluid Movement, Lymph Formation and Lymph Flow," by Philip D. McMaster, Rockefeller Institute for Medical Research, New York, N. Y.

"The Significance of Lymphatic Blockade in Immunity," by Valy Menkin, Department of Pathology, Duke University School of Medicine, Durham, North Carolina.

"The Lymphatic System and Extravascular Protein," by Cecil K. Drinker, School of Public Health, Harvard University, Boston, Massachusetts.

"The Role of the Lymphocyte in the Circulation of Lymph," by William Ehrich, Department of Pathology, University of Pennsylvania, Philadelphia, Pennsylvania.

"The Role of Lymphocytes in Normal and Immune Globulin Production and the Mode of Release of Globulin from Lymphocytes," by Abraham White and Thomas F. Dougherty, Department of Physiolog-

ical Chemistry, Yale University School of Medicine, New Haven, Connecticut.

An informal showing of motion pictures at an evening session supplemented and illustrated points made in the presentation of papers during the preceding day-time sessions, as follows:

"The Circulation of Blood and Lymphatics in the Bat's Wing," by Paul Nicoll.

"The Mesenteric and Omental Circulation," by Robert Chambers and Benjamin Zweifach.

"The Entrance of Dye into Lymphatic Capillaries of Human Skin during an Intradermal Injection," by Philip D. McMaster.

NEW MEMBERS

Elected April 26, 1945

SUSTAINING MEMBER

Brokaw, Albert D., Ph.D., Consulting Geologist. Brokaw, Dixon and McKee, New York, N. Y.

ACTIVE MEMBERS

Belanger, Alice Lois, B.S., Biology. Teacher, Eatontown, New Jersey.

Bryan, Alice I., Ph.D., Psychology. Assistant Professor, School of Library Service, Columbia University, New York, N. Y.

Dunbar, Flanders, Ph.D., B.D., Med Sc.D., M.D., Psychosomatic Medicine Associate in Psychiatry and Assistant Attending Psychiatrist, Presbyterian Hospital and Vanderbilt Clinic, New York, N. Y.

Flett, Lawrence H., B.S., Organic Chemicals. Director, New Products Division, Allied Chemical and Dye Corporation, New York, N. Y.

Gubner, Richard, M.D., Internal Medicine. Assistant Medical Director, Equitable Life Insurance Society and Instructor in Medicine, Long Island College of Medicine, Brooklyn, New York.

Harris, T. N., M.D., Immunology. Instructor in Pediatrics, School of Medicine, University of Pennsylvania; Director, Research Laboratories, Children's Seashore House, Atlantic City, New Jersey.

Holmes, Mrs. Margaret A., Geology and Mineralogy. Waldorf-Astoria Hotel, New York, N. Y.

Horkheimer, Max, Ph.D., Social Science and Social Philosophy. Research Director, Institute of Social Research, Columbia University; Research Consultant, American Jewish Committee, New York, N. Y.

Huber, Olive, Ph.D., Physiology. Instructor in Physiology, Hunter College, New York, N. Y.

Kaback, Goldie R., M.A., Personality, Vocational Guidance. Instructor in Vocational Guidance, Teachers College, Columbia University, New York, N. Y.

Kalckar, Herman M., M.D., Ph.D., Enzyme Chemistry. Research Associate, Division of Nutrition and Physiology, Public Health Research Institute of the City of New York, New York, N. Y.

King, Ellen Eva, A.M., Histo-physiology, Endocrine. Instructor, Sarah Lawrence College, Bronxville, N. Y.

Koehler, Warrent Brown, A.B., Psychology, Linguistics. Head, Department of Testing and Phonetics, Milton Academy, Milton, Massachusetts.

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Langer, Theodore William, Ph.D., Physical Chemistry, Petroleum. Project Leader, The Texas Company, Beacon, New York.

Lattes, Raffaele, M.D., Experimental Surgery and Surgical Pathology. Instructor in Surgery, Resident Surgical Pathologist, College of Physicians and Surgeons, New York, N. Y.

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Smythe, C. V., Ph.D., Biochemistry. Head of Biochemistry, Rohm and Haas Company, Philadelphia, Pennsylvania.
Spitz, René A., M.D. (N. Y.); Dr.Med. (Hungary); Dr.Med. (Czecho-Slovakia), Psychology, (experimental, clinical child development) Psychiatry, Psychoanalysis. Lecturer Instructor, Psychoanalytic Institute, New York, N. Y.
Stavitsky, Abram B., Ph.D., Bacteriology, Pathology. Bacteriologist, Henry Phipps Institute, Philadelphia, Pennsylvania.
Strong, Leonell C., Ph.D., Sc.D., Cancer, Genetics. Associate Professor, Department of Anatomy, Yale University, New Haven, Connecticut.
Warburg, Bettina, M.D., Psychosomatic Medicine. Private Practice, Psychoanalysis, New York, N. Y.

ASSOCIATE MEMBERS

Ball, Eric G., Ph.D., Biochemistry. Associate Professor, Biological Chemistry, Harvard Medical School, Boston, Massachusetts.
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Szilagyi, Inge, Psychology. Student. Brooklyn College, Brooklyn, New York.

TRANSACTIONS
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THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 7 JUNE, 1945 No. 8

SECTION OF GEOLOGY AND MINERALOGY*

MAY 7, 1945

DOCTOR LEWIS B. KELLUM, Director, Museum of Paleontology, University of Michigan, Ann Arbor, Michigan: *Jurassic Stratigraphy of Alaska and Petroleum Exploration in Northwest America.* (This lecture was illustrated with lantern slides.)

The bearing of the Jurassic stratigraphy of southwestern Alaska on the petroleum possibilities of northwestern America stems from the events of geologic history recorded in the rocks. The faunal zones recognized in stratigraphic sections of near shore marine deposits dates the orogenic, physiographic and climatic changes taking place on nearby lands and in the neritic belt. Source and reservoir rocks in the sections can be projected into the subsurface of nearby areas where the cover is sufficient to permit accumulation of hydrocarbons. Precise age determination of the rock units within the formations permits the recognition of structural features which may influence the migration of hydrocarbons. The accurate correlation of strata in southwestern Alaska with those of the Pacific coast and Rocky Mountain region of Canada and the western United States by means of the faunas, integrates the geologic events throughout northwestern America, highlights the oil producing horizons of one area for intensive study in distant

* No meeting of the Section of Anthropology was held in May.

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TABLE 1

TENTATIVE COMPOSITE TRIASSIC AND JURASSIC STRATIGRAPHIC SUCCESSION IN SOUTHWESTREN ALASKA

Period	Formation	Lithology	Faunas
		White or light-colored, hard arkosic sandstone, andesitic tuff, coarse and fine sandstone, shale and conglomerate. Thin sills of quartz diorite are intruded into the sedimentary beds. 3000+ feet.	*Aucella*
	Naknek 5000′	Gray shale with dark arkosic beds. At the base is fine conglomerate or grit with thin sandy beds scattered through it, equivalent to the Chisik Conglomerate. 1645 feet.	*Cardioceras*
		Chisik Conglomerate: Locally developed; 290′–400′. Boulders of granite or diorite and other igneous rocks embedded in tuffaceous andesitic matrix.	
		Massive black shale with some limestone lenses and nodules. 700–1000 feet.	
		Massive brown to gray sandstone with minor amounts of shale and conglomerate. 4000–4700 feet.	*Astarte* sp. E
		Gray sandy shale. 200 to 1200 feet.	
Upper Jurassic		Chinitna shale member: Gray shale with stringers of fine and coarse sandstone and buff-weathering limestone; large limestone concretions weathered ashy gray at one horizon carry fossils. A thin porphyry sill intrudes this section at Wide Bay. 400–1300 feet.	*Cadoceras*
	Shelikof 8000′	Well-stratified gray shale interbedded with thin lenticular layers of limestone and thin beds of soft gray sandstone weathered yellow; limestone concretions varying from 1 to 10 feet in longest dimension contain wood fragments and a large flat *Inoceramus*. Several diabase sills intrude these strata at Wide Bay. 950 feet.	
		Interbedded sandy shale and fine to medium grained silty sandstone containing gray limestone concretions. 100± feet.	*Seymourites*

TABLE 1 (Continued)
TENTATIVE COMPOSITE TRIASSIC AND JURASSIC STRATIGRAPHIC SUCCESSION IN SOUTHWESTERN ALASKA

Period	Formation	Lithology	Faunas
Upper Jurassic	Tuxedni 4000′	Tonnie sandstone member: a series of alternating sandstones, shales and coarse conglomerates with numerous fossil bearing horizons. 1200 feet.	*Miccocephalites*
Middle Jurassic		Sandy shale with stringers of shaly sandstone; near the middle are coarse sandstones grading into conglomerates with cobbles of volcanic rock; small calcareous concretions in the shale near the base are fossiliferous. 950 feet.	*Oppelia* sp. C
		Cynthia Falls member: Medium-grained gray sandstone grading horizontally into alternating sandstone and conglomerate. 800 feet.	
		Sandy shale with numerous stringers of shaly sandstone and with concretionary masses in the upper half; fossils present at many horizons. 1250 feet.	*Defonticeras* sp. B *Zemistephanus*
Lower Jurassic	Kialagvik 1600′	Kolosh member: (a) Platy to massive medium-grained to conglomeratic greenish-gray sandstone containing wood fragments and *Inoceramus*. 200 feet.	*Inoceramus* sp. C
		(b) Interbedded sandstone and shale becoming predominantly sandstone at the top. 300 feet.	*Dactylioceras*
		Aleuts member: (a) Interbedded sandstone, shale and conglomerate, increasing in coarseness upward with several beds of conglomerate in the upper 200 feet. 450 feet.	*Hammatoceras? kialagvikense*
		(b) Dark gray to black shale grading upward into gray shaly sandstone. 650 feet.	"*Hammatoceras*" *howelli*
	Bidarka 2300′	Dark gray-black shale with occasional thin beds of light colored, coarse-grained, well-indurated sandstone. 1300 feet.	*Arieticeras?*
		Massive to thin-bedded tuffaceous sandstone and interbedded calcareous shale and limestone. 1000 feet.	*Astarte* sp. D
Upper Triassic	Kamishak 1400′	Thin-bedded, dense, organic limestone interbedded with shale and sandstone; intruded by dikes and sills.	*Pseudomonotis subcircularis*

potentially productive areas, and shows the extent and distribution of synchronous seas, the position of their shorelines, their hinge lines of subsidence and their basins.

The sequence of Jurassic and Triassic formations in southwestern Alaska is shown in TABLE 1.

The Kamishak formation exposed at Cold Bay is dominantly limestones and may be considered favorable source rocks for petroleum. Their apparent organic character is also encouraging. The limestones are not greatly metamorphosed and the few igneous dikes and sills which penetrate them have produced only narrow zones of alteration. These intrusions are thought to be of pre-Jurassic age, since they are confined to the Triassic rocks. The overlying Jurassic strata are free from intrusives in the Cold Bay section. The gradational and apparently conformable contact between the Triassic and Jurassic systems in this section indicates that there was no period of folding, uplift and erosion after the Triassic limestones and associated sandstone had been deposited. Such an unconformity, if present, might have afforded the opportunity for escape of liquid hydrocarbons which had accumulated in the Triassic rocks before Jurassic time. On the other hand, many if not most large accumulations of oil throughout the world are associated with unconformable contacts. Tuffaceous sandstones in the upper part of the Triassic section furnish possible reservoirs for the accumulation of oil formed in the limestone and shale source rocks. The objective of any deep well drilled in the Alaska Peninsula should be to penetrate the Triassic rocks from top to bottom. It is perhaps significant, however, that the Triassic throughout the world has produced less oil than any other post-Cambrian system. This is explained by the fact that it was a time of restricted seas and expanded deposition of continental and volcanic deposits. These presumably lacked the kind and quantity of organic material to yield the hydrocarbons necessary to form commercial quantities of petroleum in the areas of the present land masses. The neritic belt in which the proper balance of highly organic marine sediments interfingered with coarse clastics to produce both source and reservoir rocks was, for the most part, during Triassic time, outside the margins of the present continents. Nevertheless, Triassic or Permo-Triassic strata have produced small quantities of oil in the Rocky Mountain region of the United States (Moenkopi and Chugwater formations), in the Mendoza Province of Argentina and the Emba district of the U. S. S. R.

Another factor bearing on the presence of commercial oil in Triassic rocks in southwestern Alaska is the igneous activity associated with them. In the Cold Bay section, the amount of igneous rock penetrating the limestones of the Kamishak formation is unimportant, but at Ursus Cove, 180 miles to the northeast, considerably more intrusive rock is present. Possibly, the igneous activity of early Mesozoic time on the Alaska Peninsula may have been decreasing in intensity to the southwest.

The Jurassic section at Cold Bay consists of 2600 feet of dark colored shales and tuffaceous sandstones. They are considered to be a favorable association of possible source and reservoir rocks. Shales predominate near the top of the formation and would serve as a cap rock for the more porous sandstones. The absence of limestones as a potential source rock in the Jurassic is to be noted, but this deficiency may be, in part, offset by their presence in the underlying Triassic, separated only by a gradational contact.

All of the sediments referred to the Jurassic in the Cold Bay section are assigned to the Lower Jurassic and are overlain disconformably by the basal conglomerate of the Upper Jurassic Shelikof formation. There is no positive evidence that the Kialagvik formation or equivalent strata are present in the Cold Bay section, although the upper 1300 feet of the Bidarka formation did not yield any fossils. The stratigraphic interval between the Kialagvik formation cropping out on the Wide Bay anticline and the Lower Jurassic Bidarka formation cannot be estimated. The absence of igneous rocks in the Jurassic at Cold Bay is favorable. The succession of fine and coarse grained sediments in the Triassic and Jurassic section implies an oscillating sea without important mountain building nearby.

There are several factors in the stratigraphic section exposed at Wide Bay which seem to have a bearing on the oil possibilities of the Alaska Peninsula. A careful comparison of the lithologic variations vertically within each faunal zone and subzone brings out the cyclic nature of the sedimentation. Within each time interval, the sediments laid down grade from finer or coarser-grained clastics. If this persists in the subsurface section, it would provide an alternation of potential source and reservoir rocks. The coarsest sdiments in the Kialagvik formation are the conglomerates interbedded with sandstones in the upper part of the Aleuts member. The member has an exposed thickness of 1150 feet and grades upward from shales in the lowest outcrops

to conglomerates at the top. At Fossil Bluffs on Wide Bay, there is a thin zone of limestone 575 feet above the base. This sequence of lithologic units interpreted in terms of shoreline development implies a regressing sea. As the shoreline moved closer to the Wide Bay area, coarser clastics were deposited. Conversely, during an earlier stage of the regression, the area was further off-shore where clearer waters prevailed, and the deposition of calcium carbonate would not have been masked to such an extent by the influx of clastics. Hence, limestones might be expected to comprise a larger percentage of the subsurface section than is present in the exposed part of the Kialagvik formation. This is supported by the repeated record of stringers of impure limestone in calcareous shale throughout the lower half of the log of Standard Grammar Well No. 1, in strata included in the Kialagvik by the Company's geologists. This well was located in the Kanatak District, 30 miles northeast of the sections measured at Wide Bay.

The Kialagvik formation on the Iniskin-Chinitna peninsula increases in coarseness from the base upward. The lower 1600 feet is chiefly sandy shale and siltstone with some sandstone stringers. The upper 900 feet consists of sandstones with numerous thick beds of cobble conglomerates derived largely from volcanic rocks. The sequence here confirms the evidence of a regressing sea and the approach of the shoreline. The coarse conglomerates suggest that this regression was accompanied by orogenic disturbances nearby, but there is no evidence of angular unconformity between the Kialagvik and the overlying Tuxedni sandstone.

The Tuxedni standstone is a series of alternating sandstones and sandy shales. The sandstones become thicker and coarser in the upper part of the section and there are several thick beds of boulder conglomerates present. The boulders in some of these are coarse granites evidently derived from the erosion of an intrusive mass brought to the surface by uplift and degradation. The beginning of Tuxedni deposition records a new transgression of the sea in Middle Jurassic time, followed by oscillations of the shoreline as deposition and erosion contended for the littoral zone. Following the initial invasion, the record is one of halting withdrawal throughout the remainder of Middle Jurassic time. The thickness of the Tuxedni sandstone indicates a subsidence of over 4000 feet in the Iniskin Chinitna Peninsula area, but the deposition of clastics kept pace with it and the sea remained shallow

throughout the period. When subsidence ceased, permanent deposition came to an end.

At Wide Bay, the Middle Jurassic Tuxedni sandstone is not present and the Upper Jurassic Shelikof formation rests on the Lower Jurassic Kialagvik without apparent unconformity. Evidently, there was little or no subsidence in the Wide Bay area during Middle Jurassic time. It began again in early Upper Jurassic time and the Shelikof sea transgressed a long distance west of the Wide Bay area where fine clays and limes were deposited. The influx of clastics from a rising land area on the northwest soon pushed the shoreline eastward in spite of several thousand feet of subsidence during the early Upper Jurassic time. The middle part of the Shelikof formation consists of sandstones and siltstones.

The Chinitna shale, which overlies the Tuxedni sandstone on the Iniskin-Chinitna Peninsula, is in part equivalent to the lower shales of the Shelikof formation and, like it, records a period of marine transgression. The overlying Naknek sandstone shows another influx of great quantities of sands, from the adjacent land-mass. These accumulated as rapidly as the subsiding sea bottom permitted their permanent deposition. The thick bed of coarse conglomerate widely present at its base records the sudden uplift which accompanied the withdrawal of the sea after the Chinitna-Lower Shelikof transgression.

The depositional record of Jurassic time in southwestern Alaska is one of great subsidence in the neritic belt, accompanied by oscillation of the shoreline. The subsidence here was balanced by uplift of the land area to the northwest contributing coarse clastics to the eastward flowing streams. Igneous activity accompanying this orogeny is reflected in the Arkosic sandstones throughout the section and the cobbles and boulders of volcanic and intrusive rocks at many horizons. The correlation of Jurassic formations and faunas of southwestern Alaska with those of Canada and the western United States is shown in TABLE 2.

TABLE 2

CORRELATION CHART OF JURASSIC FORMATIONS AND

<table>
<tr><th>PERIOD</th><th>EUROPEAN STAGE</th><th>AGE IN BUCKMAN'S CLASSIFICATION (AFTER McLEARN)</th><th>WIDE BAY AND COLD BAY ALASKA</th><th>FAUNAS OF SOUTH-WESTERN ALASKA</th></tr>
<tr><td rowspan="8">UPPER JURASSIC</td><td></td><td rowspan="5"></td><td rowspan="2">Naknek SS</td><td>Aucella</td></tr>
<tr><td>Argovian</td><td>Cardioceras</td></tr>
<tr><td rowspan="3"></td><td rowspan="4">Shelikof Fm.</td><td></td></tr>
<tr><td>Astarte sp. E</td></tr>
<tr><td></td></tr>
<tr><td rowspan="3">Callovian</td><td rowspan="2">Propanulitan</td><td rowspan="2">Cadoceras
Seymourites
Miccocephalites</td></tr>
<tr><td rowspan="9">HIATUS</td></tr>
<tr><td>Macrocephalitan</td><td rowspan="3"></td></tr>
<tr><td rowspan="6">MIDDLE JURASSIC</td><td rowspan="8"></td><td></td></tr>
<tr><td>Stephanoceras</td></tr>
<tr><td rowspan="4">Sonninian</td><td>Oppelia sp. C</td></tr>
<tr><td>Defonticeras sp. B</td></tr>
<tr><td>Zemistephanus</td></tr>
<tr><td rowspan="2"></td></tr>
<tr><td rowspan="5">LOWER JURASSIC</td><td></td></tr>
<tr><td>Hildoceratan</td><td rowspan="2">Kialagvik Fm.</td><td>Dactylioceras</td></tr>
<tr><td>Toarcian</td><td rowspan="3"></td><td>"Hammatoceras"</td></tr>
<tr><td></td><td rowspan="2">Bidarka Fm.
?</td><td rowspan="2">Arieticeras ?
?</td></tr>
<tr><td>Sinemurian</td></tr>
</table>

FAUNAS IN SOUTHWESTERN ALASKA, CANADA AND OREGON

INISKIN-CHINITNA PENINSULA	FAUNAS OF PACIFIC COAST OF CANADA		FAUNAS OF FERNIE FORMATION OF CANADA	OREGON
Naknek SS.				?
			Cardioceras canadense	Lonesome Fm.
			Peltoceras occidentale	
				?
Chinitna Sh.				
	Yakounites	Y	Seymourites	Trowbridge Group
		A	Corbula munda	
	Toricelliceras	K		
		O	Chlamys mcconnelli	
	Teloceras	U	Teloceras, Stemmatoceras, Defonticeras, Saxiton-iceras, Zemistephanus	Izee Group
Tuxedni SS.		N		
	Defonticeras			
	Zemistephanus; Kanastephanus			Colpitts Group
			Sonninia	
?				
Kialagvik Fm.	"Dactylioceras"	M	Dactylioceras; Hammatoceras	
		A		Mowich Group
		U	Grammoceras	
	"Sequinziceras"	D		Donovan Group
	"Oxynoticeras"	E		
			Eparnioceras	

SECTION OF BIOLOGY

May 14, 1945

Doctor J. S. Kiser and Doctor J. S. Zellat, Lederle Laboratories, Pearl River, N. Y.: *Antibiotics, Other than Penicillin, Produced by Penicillia.* (This lecture was illustrated with lantern slides.)

Since it was evident, from the beginning, that penicillin would be ineffective against a large number of microorganisms, particularly the Gram negative bacteria which are the cause of such diseases as typhoid fever, cholera, undulant fever, bacillary dysentery and plague, many microbiologists turned their attention to seeking other antibodies which would be effective in treating these diseases.

The search for these substances has, by no means, been limited to the *Penicillia,* but has included a great portion of the plant kingdom, and antibiotic substances have been demonstrated from such diverse sources as onion juice and the extracted growth of *Cheatomium,* a saprophytic soil fungus. Most of these substances, however, have been either too toxic for medical use, ineffective in the animal body, or only weakly antibiotic. The *Penicillia* have yielded a number of compounds, all of which fall into one or another of the above categories, but which, nevertheless, are of interest for various reasons. It is with these substances that this talk is concerned.

The search for antibiotic substances among the *Penicillia* consists of three steps: First, a rapid, qualitative test of large numbers of cultures for any degree of antibiotic activity; second, the production of a suitably high concentration of the substance in a medium from which it may be extracted and purified; and third, the isolation and purification of the substance, its identification, if it is a known compound, or the testing of the compound for toxicity and, finally, for ability to protect animals against infection and cure them after infection.

The first of these steps, the testing of large numbers of cultures for antibiotic activity, may be satisfactorily conducted by growing a single colony of the mold on a suitable medium on a Petri plate and streaking the test organism from the edge of the plate to the colony. This method permits the use of several test organisms and the testing of the same culture at several times for antibiotic activity, since it is known that some antibiotics may be produced, then later destroyed, in the medium.

The medium used must be suitable, not only for the growth of the

mold and the production of an antibiotic, two conditions, which may differ widely; but also for the growth of several different kinds of bacteria.

A suitable medium for this purpose is a 1% peptone, 0.5% meat extract, medium + 0.3% K_2HPO and 2% sucrose.

The organisms commonly used comprise both Gram positive and Gram negative species. *Escherichia coli, Staphylococcus aureus, Bacillus subtilis* and *Eberthella typhosa* are used commonly.

A loopful of a spore suspension of the culture to be tested is placed in the center of the plate and the plate incubated at about 23°–24° C. for 5–7 days, depending on the culture. The plate is then streaked from the edge of the plate to the edge of the colony with each test culture, and the plates are further incubated at 37° C. for 24 hours when they are observed for growth of the test organisms. They are then replaced in the 24° C. incubator for 5–7 days, retested, and sometimes even tested after a third period of incubation.

After a culture is found to have antibiotic activity, the next step is to produce the substance in a concentration and in a medium from which it may be extracted and purified.

Since the antibiotic may not be produced in a liquid medium of the same composition as the agar on which it was originally found, it is advisable to test a number of media. Some media commonly used are those of Raulin-Thom and the Czapek-Dox medium. These may be supplemented with various materials, such as apple juice concentrate, malt extract, beet molasses, corn steep liquor, asparagus press juice concentrate, soy bean meal, etc. Yeast extract, a favorite source of nutrilites for bacteria, has proven disappointing as an accessory substance to stimulate antibiotic production. It is also advisable to incubate a duplicate set of cultures on a shaker, since some antibiotics are produced in higher concentration in shaken cultures than in still, i.e. surface, cultures. The problem of assaying these mold cultures for antibiotic activity can be met in several ways. A commonly used method is to incorporate a sample of the culture fluid into a suitable medium, either a broth or an agar medium. Serial dilutions are made in this medium. In the case of a broth medium, only a single organism can be used per tube of medium; thus, the agar has an advantage, since it may be poured into a Petri plate and a streak inoculation made with each of several organisms. This provides an estimate of the relative sensitivity of the various test organisms for the antibiotic. This

bacterial spectrum, as Waksman has termed it, is of great importance in identifying the substance, since each of the known antibiotics has a quite characteristic bacterial spectrum. This will be further discussed in connection with each substance. It is not always necessary to sterilize the sample of culture fluid used, since the assay is usually carried out at 37° C. and the mold may not grow enough during the incubation at that temperature to interfere with the reading of the results. Furthermore, sterilization of the sample often presents a real problem since many antibiotics, notably penicillin and gliotoxin are heat-labile, while others, like streptomycin, are adsorbed to some extent by ordinary methods of filtration. It may sometimes be practical to combine centrifugation and pasteurization with some antibiotics.

Once a suitable medium and method of producing a high concentration of activity has been obtained, a relatively large batch of medium is prepared, usually twenty to fifty liters. This is distributed in amounts of approximately 250 ml. per flask in suitable flasks, inoculated and incubated for several days, when samples are taken for assay. When these assays reveal a relatively high concentration of the antibiotic, the fluid is harvested. Here the real problem begins, for the isolation, purification and identification of the antibiotic is often a laborious and time consuming process, and, since it differs with each substance, it is, perhaps, best to take up these methods in the discussion of the individual antibiotics.

Eight separate identified antibiotics have already been reported as being produced by *Penicillia.* The structural formula of four of these, clavacin, penicillic acid, citrinin, spinulosin has been determined. Puberulic and puberulonic acids and penicillin and penatin are still unknown.

Clavacin

Of all the antibiotics, other than penicillin, so far described as being produced by *Penicillia,* the most interesting is clavacin. This substance has been isolated from a number of *Penicillium* species and has been given a variety of names. It is the claviformin from *P. claviforme* of Chain *et al.,* the patulin from *P. patulum* of Raistrick, and, like most of the other antibiotics, it has also been isolated from other genera. In fact, the name most commonly used in this country, clavacin, is derived from *Aspergillus clavatus,* the species used by Dr. Waksman in his first work with it. Clavacin is a very powerful and versa-

tile antibiotic, but, unfortunately, it is also very toxic. Nevertheless, it has received considerable study in the hope that, since its structure is known, the toxicity might be modified without destroying its effectiveness as a bacteriostatic and bactericidal agent. Raistrick first published the structure of clavacin, which is known as patulin in England, in the Lancet, in November, 1943.[1] Chemically, it is anhydro-3-hydroxy-methylene-tetrahydro-γ-pyrone-2 carboxylic acid. In our experiments, it was the only antibiotic produced in all the media used. It produced a comparatively high titer against all the test organisms and no difficulty was encountered in preparing large batches of active filtrate.

The filtrate was reduced in volume *in vacuo* to about 1/20th the original, then continuously extracted with ether for 24 hours. The ether extract was concentrated and set in the chill room at 5° C. overnight, when large quantities of fine white crystals had precipitated. This material was filtered off, recrystallized from ether, and the melting point and chemical composition determined.

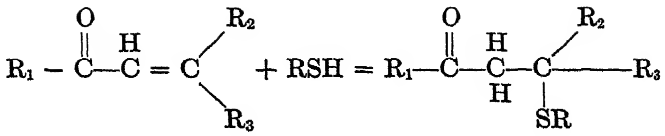
m. p. 109°

$C = 54.37, \; H = 4.27$

These values compare well with the melting point 109–110° C. uncorrected and analysis C 54.58, H 4.28% of the St. Louis University Medical School Group[2] and with the theoretical for $C_7H_6O_4$; $C = 54.54, H = 3.90\%$.

Since the results of our *in vitro* tests and chemical tests were similar to those of Raistrick, Hooper and others, we did not, at this time, carry out extensive degradation processes for the identification of the product but did do some toxicity tests. 0.2 mg., intraperitoneally, was the LD_{100} for 20 gm. mice. The value for similar tests by the St. Louis group was 0.1 mg. The most striking symptom of toxicity with this substance is the terrific edema, resulting in hemoconcentration. This is so great that, upon death of the animal, the lungs may be filled with fluid to the point where they will not float.

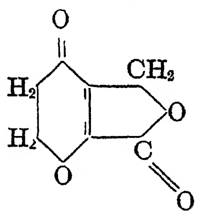
The mode of action of clavacin has been the subject of considerable study and Geiger and Conn[3] have suggested that it may be due to the inactivation of sulfhydryl groups. The exact process is unknown, but it is known that clavacin is an α-β-unsaturated ketone which will react with cysteine, thioglycollate or glutathione and, thereupon, loses its bacteriostatic activity. The reaction postulated by Posner is as follows:



If this is actually the mechanism of the reaction, it would be easy to understand the extreme toxicity of the compound, because of the known requirements of many organisms for sulfhydryl groups, but it leaves unexplained the vast difference in toxicity between clavacin and penicillic acid, which also reacts with and is inactivated by sulfhydryl groups.

The active α-β unsaturated ketones are characterized by the fact that R_1 is an aromatic group, such as the phenyl group, and either R_2 or R_3 is an H group. Clavacin, but not penicillic acid, is inactivated by thiosulfate. Acrylophenone, benzalacetophenone and furfuralacetophenone were active bacteriostatic agents, but only acrylophenone reacted with the sulfhydryls.

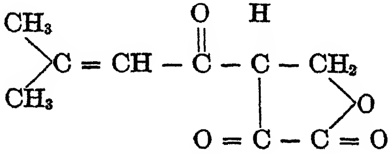
The problem of the synthesis of clavacin has been studied in a number of laboratories and recently Peutzer, Nield and Barry[4] have published the synthesis of a clavacin isomer.



Compound I

This substance was reported as showing a slight bacteriostatic activity *in vitro* against *Staphylococcus aureus* at a 1:2000 dilution. They also state that Compound I absorbs only 1 mole of hydrogen under conditions under which clavacin absorbs from 3–4 moles. The double bond in the pyrone ring is not saturated, as in the case of clavacin. This would seem to disprove the suggestion of Bergel *et al.* that a dynamic tautomerism exists between clavacin and Compound I.

They also reported that



Compound II

a compound formed by the opening of the pyrone ring of the dimethyl homologue of clavacin, had bacteriostatic activity in a dilution of 1:4000. No mention is made of inactivation of this compound by sulfhydryl groups, i.e., whether it gave a positive nitroprusside test after treatment with — SH compounds, but it should be pointed out that this compound is also an α-β unsaturated ketone.

Penicillic Acid

This substance was originally isolated in 1913 by Alsburg and Black from a culture of *Penicillium puberulum* Bainier.[5] It has since been described from *P. cyclopium* Westling by Birkinshaw, Oxford and Raistrick[6] and from *P. Thomii* and *P. suavolens* by Karow, Woodruff and Foster.[7]

It is active against both Gram positive and Gram negative organisms in concentrations from three to ten times as great as the concentrations of clavacin required for the same organism under the same conditions. However, it is much less toxic, so is of some interest.

It was produced in Raulin-Thom medium. The medium was concentrated *in vacuo* to about 1/20 the original volume and continuously extracted with ether for 48 hours. The ether extract was further concentrated *in vacuo* and set in the chill room overnight. A large quantity of crystals had precipitated. They were recrystallized from petroleum ether and the melting point and analysis were done. The melting point 81–84° C. compares well with the values reported by Raistrick, 86° C. for the anhydrous acid and the composition: $C = 56.42$, $H = 5.85$ is nearly identical with the theoretical value for $C_8H_{10}O_4$, i.e., C 56.44 and H 5.93. The bacteriostatic activity of penicillic acid compared with that for clavacin was as follows:

TABLE 1
BACTERIOSTATIC CONCENTRATIONS, mg %

	Penicillic acid	Clavacin
Salmonella pullorum	10	0.5
Brucella abortus Strain 19	20	0.65
Eberthella typhosa	15	1.25
Vibrio cholerae	15	1.25
Shigella dysenteriae	25	1.25
Pasteurella bubalseptica	10	0.65
Shigella gallinarum	15	1.25
Escherichia coli	>25	2.5
Salmonella cholera-suis	20	2.5
Staphylococcus aureus	20	1.25
Streptococcus hemolyticus	10	<0.5
Bacillus subtilis	25	5.0

The toxicity level was determined in 12 day chick embryos to be 1.5 mg. as LD_{50}. The material was injected into the allantoic cavity. Oxford, Raistrick and Smith gave 300 gm/kg as the lethal subcutaneous dose in 20 gm. mice.

Spinulosin

Spinulosin, 3,6 dihydroxy, 4 methoxytoluquinone, was first isolated from *Penicillium spinulosum* Thom by Birkinshaw and Raistrick in 1931.[8] The structure was confirmed by synthesis.

Spinulosin is only weakly antibiotic being effective against *Staphylococcus aureus* in a dilution of about 1:6000. It is almost equally effective, however, against Gram negative bacteria. It is further of interest, because a closely related substance, fumigatin, was isolated by Anslow and Raistrick from filtrates of *Aspergillus fumigatus* Fresenius cultures. This substance, whose structure has also been confirmed by synthesis, is 3 hydroxy, 4 methoxy toluquinone. These investigators found that not all strains of *Aspergillus fumigatus* produced fumigatin but that spinulosin was produced by some. Following the lead provided by the substances produced by these two organisms, Raistrick's group synthesized and tested a number of substituted benzoquinones and toluquinones. P-benzoquinone is, itself, a fairly potent anti-bacterial substance, capable of completely inhibiting growth of *Staphylococcus aureus* for 24 hours in glucose broth at a dilution of 1:12000. Its activity is diminished by preliminary incubation in glucose broth at 37°, as is the activity of all of the toluquinones and benzoquinones.

Two other toluquinones which have been found to be actively bacteriostatic are 4 methoxy and 4:6 dimethoxytoluquinone. The first of these will inhibit *Staphylococcus aureus* at a dilution of 1:300,000 for 24 hours. The dimethoxy compound is even more effective, since it inhibits the *Staphylococcus* at a dilution of 1:500,000. It might be pointed out, at this time, that the activity of the toluquinones seems to be connected with the methoxy groups and that hydroxy groups reduce the activity. Thus, spinulosin, the 3,6 dihydroxy monomethoxy compound, is less active than fumigatin, the 3 hydroxy monomethoxy compound, which is, in turn, less active than the unsubstituted monomethoxy toluquinone, while the dimethoxy compound is most active of all. Unfortunately, the toxicity of the compounds increases in the same order and the 4,6 dimethoxytoluquinone is too toxic to be of any

therapeutic value. Moreover, a protection test reported by Miss Barber, in which mice were inoculated with *Streptococcus pyogenes,* then treated with 4,6 dimethoxytoluquinone, showed no protection.

Penatin

Penatin, first described by Kocholaty,[9] is not an antibiotic in the true sense of the word, since it achieves its effect indirectly by a catalytic action, rather than by any intrinsic properties of its own. It is an enzyme, specifically a glucose dehydrogenase, and its antibacterial activity is due to the formation of hydrogen peroxide. The reaction is as follows:

$$\text{Glucose} + H_2O + O_2 \rightarrow \text{Gluconic acid} + H_2O_2$$

Since the antibacterial action is actually due to hydrogen peroxide rather than to the enzyme, the range of activity is wide and includes both aerobes and anaerobes, Gram positive and Gram negative forms.

Kocholaty adsorbed the material on kaolin at an acid pH, eluted with pyridine, precipitated with dioxane and, finally, took it up in water. The St. Louis group[10] used a much simpler method involving precipitation with uranium acetate and liberation with phosphate at pH 6.8. The enzyme is then salted out with ammonium sulfate, dialyzed to get rid of the sulfate, and lyophilized. Recovery of activity is practically complete.

The prosthetic group of the enzyme isolated by the St. Louis group was shown to be flavine adenine dinucleotide. Their preparation differed somewhat from that of the group at the London School of Hygiene & Tropical Medicine who referred to their substance as notatin. This difference might be due to a slight difference in the enzyme. The glucose dehydrogenase, isolated at this laboratory, was essentially the same in its action as that reported by the St. Louis group. It has been shown that the antibacterial activity is wholly accounted for by the hydrogen peroxide produced. Gluconic acid will prevent the growth of *Staphylococcus aureus* in a 1:1000 concentration in a peptone-glucose medium, but this concentration lowers the pH to 4.2. Inhibitory concentrations of penatin do not depress the pH below 6.2, which is not, in itself, inhibitory. The concentration of the enzyme, as well as that of the glucose, effects the amount of the hydrogen peroxide formed and therefore the amount of antibacterial activity exhibited by the preparation. Catalase completely destroys the activity of the

substance and, of course, fresh unheated serum also decreases or destroys the activity, though old, or heated serum will cause little or no decrease in antibacterial activity. The enzyme, itself, is not inactivated by potassium cyanide, so it is possible to mix the enzyme with a mixture of 0.1 M KCN and catalase, which is inactivated by the cyanide, and have the preparation retain full activity. Moreover, the antibacterial effect may be destroyed by any chemical means which will destroy a corresponding amount of hydrogen peroxide. These include the addition of ferrous salts, cysteine, and sodium thiosulfate.

A few toxicity and protection tests were run on this substance. Kocholaty reported that 500 mg., intramuscularly, or 20 mg. per day for five days was not toxic to guinea pigs, but other workers found the toxicity of their preparations to be much greater. No protective effects have been reported. This substance is of importance because it may simulate an active antibiotic. It can be rather easily ruled out, however, by a few simple tests. First, is the substance active in glucose free media? In this connection, one should not overlook the fact that glucose may be carried over in the filtrate sample and even 0.005% of glucose may produce significant amounts of hydrogen peroxide. However, if the material is significantly more active in a glucose containing medium than in one lacking glucose, the effect of catalase or of fresh serum upon the activity should be determined. If the activity is greatly decreased or destroyed, two other simple tests may be applied. Precipitation with uranium acetate and elution with phosphate at pH 6.8 plus failure to dialyse can be considered pretty conclusive evidence that the substance in question is a glucose dehydrogenase.

Citrinin

First isolated in 1931 by Hetherington and Raistrick[11] from a culture of *Penicillium citrinin* Thom, it is probably the easiest of the antibiotics to prepare, since it is only necessary to acidify the filtered culture in order to precipitate the yellow microcrystalline compound. The precipitate is filtered, washed and recrystallized from boiling absolute ethanol.

Citrinin is principally effective against Gram positive bacteria which it inhibits in high diluton. It has not proven to have any *in vivo* activity, though it is not excessively toxic.

Puberulic and Puberulonic Acids

These substances are found in small amounts in cultures of *Penicillium puberulum.*[12] Puberulic acid is a practically colorless dibasic acid which is bacteriostatic for Gram positive bacteria in low concentrations. Puberulonic acid, which is bright yellow, is a much less active bacteriostatic substance. No reports on toxicity or protective action have appeared.

BIBLIOGRAPHY

1. **Raistrick, H., J. H. Birkinshaw, A. Bracken, & S. E. Michael.**
   1943. Lancet **2:** 625.
2. **Katzman, P. A., E. E. Hayes, C. K. Cain, J. J. Van Wyk, F. J. Reithel, S. A. Thayer, E. A. Doisy, W. L. Gaby, C. J. Carroll, R. D. Muir, & L. R. Jones.**
   1944. J. Biol. Chem. **154:** 475.
3. **Geiger, W. B., & J. E. Conn.**
   1945. J. Am. Chem. Soc **67:** 112.
4. **Puetzer, B., C. H. Nield, R. H. Barry.**
   1945. Science **101:** 307.
5. **Alsberg, C. L., & O. F. Black.**
   1913. U. S. Dept. Agr. Bureau of Plant Ind., Bull. **270.**
6. **Birkinshaw, J. H., A. E. Oxford, & H. Raistrick.**
   1936. Biochem. J. **30:** 394.
7. **Karow, E. O., H. B. Woodruff, & J. W. Foster.**
   1944. Arch. Biochem. **5:** 279.
8. **Birkinshaw, J. H., & H. Raistrick.**
   1931. Phil. Trans. Royal Soc. London (B), **220:** 245.
9. **Kocholaty, W.**
   1942. J. Bact. **44:** 143.
10. **Van Bruggen, J. T., F. J. Reithel, C. K. Cain, P. A. Katzman, E. A. Doisy, R. D. Muir, E. C. Roberts, W. L. Gaby, D. M. Homan, & L. R. Jones.**
    1943. J. Biol. Chem. **148:** 365.
11. **Hetherington, A. C., & H. Raistrick.**
    1931. Trans. Royal Soc. London (B) **220:** 269.
12. **Birkenshaw, J. H., & H. Raistrick.**
    1932. Biochem. J. **26:** 441.

SECTION OF PSYCHOLOGY

May 21, 1945

Doctor Morris S. Viteles, Professor of Psychology, University of Pennsylvania, Philadelphia, Pa.; Chairman, National Research Council Committee on Selection and Training of Aircraft Pilots: *Research in Aviation Psychology*. (This lecture was illustrated by lantern slides.)

In 1939, the Civil Aeronautics Authority (now the Civil Aeronautics Administration) undertook an ambitious program of training civilian pilots. The purpose of this was to make young men and young women airminded and to prepare the present generation of young people to fly the private and commercial planes of the future. This program, known as the Civilian Pilot Training Program, was operated through the universities of the country, making use of already established facilities in the hands of private operators while encouraging others to set up similar facilities.

The first phase of the Civilian Pilot Training Program called for instruction of 10,000 pilots, which was quickly expanded to the number of 50,000. Almost simultaneously with the formulation of the program, largely through the efforts of Robert Hinckley, at that time Chairman of the Civil Aeronautics Authority, and Dean R. Brimhall, Director of Research, funds were set aside for research on selection and training of civilian aircraft pilots. This was done in the belief that an extended program of civilian pilot training should make use of every possible scientific aid for selecting those most competent to fly; for determining the best methods of training; for the appraisal of flight achievement; and for safeguarding the adjustment of the pilot. Moreover, it was recognized that this large scale training program provided unusual opportunities for renewing the study of problems related to the human side of aviation which had been initiated during World War I and abandoned almost immediately with the signing of the Armistice.

The National Research Council was asked to undertake the responsibility for administering these research funds, and a committee, known as the Committee on Selection and Training of Civilian Aircraft Pilots, with J. G. Jenkins as Chairman, was set up to organize and supervise the research program. This committee, which includes psy-

chologists, physiologists, physicians, and engineers, has been in continuous operation since 1939, and, through a small Executive Subcommittee, has conducted research in aviation psychology involving the expenditure of well over three-quarters of a million dollars. Early in 1941—with some foresight—the word "civilian" was dropped from the name of the Committee, and close liaison was established with military services who nominated representatives to become members of the Executive Subcommittee. Since that time, the work of the Executive Subcommittee has been conducted in close association with the Army and Navy and many of the results of the Committee research have been applied by the Services.

As in the application of psychology in similar situations, such as in industry or education, the research program of aviation psychology has been organized around the three basic functions of selection, training, and maintenance of personnel.

Since the first objective of the initial phase of the pilot training program was to build up a large pool of competent pilots, it was natural to start research with investigations bearing on the problem of selecting, from a random group of applicants, those men who were particularly qualified to learn to fly. Investigations were undertaken simultaneously in many university centers where civilian pilot training programs were under way. In addition, in cooperation with the Navy, the Committee had an opportunity to make a highly extensive study at a Naval training station where approximately 800 aviation cadets and instructors were available as subjects.[1] In this investigation, a very large number of psychological tests, both of the paper-and-pencil and of the psychomotor type; a large variety of physiological tests, including the tilt-table, the electrocardiogram, electroencephalogram and others; and, in addition, the personal interview was subjected to experimental study. Concurrently, individual predictors were subjected to intensive studies at other centers and, later, integrated projects involving the examination of civilian pilot trainees at Boston and in the Midwest area were employed to check the findings of earlier investigations.

In the area of psychological tests, it was found that two easily administered paper-and-pencil tests could effectively be used in reducing the proportion of failures in the primary phase of pilot training. These included a standard mental alertness test and a biographical informa-

tion blank through which the interests, the family background, the occupational experience, and avocational activities of the candidate were explored. Later work, done both by the Committee and the Navy, showed that still a third test—a test of mechanical comprehension—could be usefully added to the battery. While it is not permissible to state the reduction in failures accomplished through the use of these tests—because this is still classified information—it is possible to say that the results were sufficiently good to lead the U. S. Navy to adopt these as the basic instruments for the selection of Naval Aviation Cadets.

Repeated studies on larger and larger samplings in the Navy gave repeated verification that the test results did bear a direct relation to the prediction of success in learning to fly at the primary level, and while changes have been made in the way of developing new forms of these tests, the research initiated by the Committee on Selection and Training of Aircraft Pilots, through the foresight of the Civil Aeronautics Administration, has furnished the backbone of the Naval Aviation Cadet selection program.

In addition to the paper-and-pencil tests, a number of psychomotor tests have proven to be extremely useful in the prediction of flying success. In passing, it should perhaps be again pointed out that flying success, as used in this address, refers to success in learning to fly the plane. So far, it has not been able to obtain the data necessary to reveal the relationship between predictors and success in the actual combat situation. However, combat data are being assembled by the military services, and it will be possible, before this war is ended, to obtain significant information on such relationships.

Psychomotor tests, which have held up for pilot selection, include a two-hand coordination test requiring coordination analogous to that of lathe operation, and an eye-hand coordination test, in which the hand is required to follow a moving visual stimulus. Among psychomotor tests which have proven to be particularly satisfactory, is one developed during the last war, the Mashburn Test, which simulates certain aspects of the task of flying, in that the subject is required to make adjustments of an airplane control stick and rudder pedal in response to visual stimuli. This test, which is essentially of the complex reaction type, when combined with the eye-hand and the two-hand coordination test, gives substantial correlation with a rigorous criterion of success in learning to fly.

Research on psychomotor instruments has been conducted, not alone by the Committee on Selection and Training of Aircraft Pilots, but also by the Army Air Forces and, somewhat in contrast to the Navy test battery, psychomotor tests are major items in the battery for the classification of personnel used by the Army Air Forces.

The selection research program of the Committee on Selection and Training of Aircraft Pilots has been eclectic in character. Since, in military, as well as in industrial situations, considerable attention has been given to the interview, it seemed well to investigate efficiency of this device in predicting the success in learning to fly. An experiment was therefore designed in which a Board of Interviewers conducted a relatively standardized interview involving the use of a carefully devised rating scale (EXHIBIT 1). Agreement among interviewers in

EXHIBIT 1

INTERVIEW CHART

Name of Candidate..............................

Name of Rater..............................

C. GENERAL SOCIAL ADJUSTMENT AS RELATED TO FLYING

25	20	15	10	5	1

ESPECIALLY WELL ADJUSTED	WELL ADJUSTED	FAIRLY WELL ADJUSTED	POORLY ADJUSTED	VERY POORLY ADJUSTED

.. is very popular;
.. has many friends;
.. gets along in most situations;
.. does not get along with people;
.. is a "lone wolf";
.. is sought out by many people

.. gets along well with others;
.. is socially like most people;
.. is a stay-at-home;
.. antagonizes people;
.. is very much interested in group activities

.. is a "joiner";
.. is a misfit;
.. would make an ideal friend for anyone;
.. is an excellent mixer;
.. has antisocial tendencies;
.. is out of touch with the world

EXPLANATION OF RATING:

predicting success or failure proved to be quite high, mean correlations among independent interviewers in rating "Fitness for Flight Training" being close to .90 (EXHIBIT 2). The interview fared surprisingly well in predicting certain objective criteria of competence in flying. Indeed, in so far as pilot performance can be measured objectively, the

EXHIBIT 2

THE CORRELATION BETWEEN THE MEAN OF THE RATINGS FOR A GIVEN INTERVIEWER ON THE FIRST 8 SCALES AND THE OVER-ALL OR "FITNESS FOR FLIGHT TRAINING" RATING FOR EACH INTERVIEWER

School	*Rater*	r_{MI}	r_{MI}
Harvard	#1	.80	
	#2	.88	
	#3	.89	
	All Three Raters		.86
Ohio	#1	.90	
	#2	.93	
	#3	.88	
	All Three Raters		.90
Purdue	#1	.86	
	#2	.84	
	#3	.85	
	All Three Raters		.83
Michigan	#1	.89	
	#2	.93	
	#3	.90	
	All Three Raters		.91

interview reached levels of prediction which are accepted as having practical significance.[2]

The interview met the routine tests of scientific acceptability. However, it failed of practical justification on a most basic and critical point. The interview, a technique which requires the services of several individuals to obtain a rating for one individual at a time, is very expensive with regard to time, personnel, and money. To be accepted as having practical usefulness in selecting pilots, it must add significantly to the predictive efficiency that is obtainable by the application of pencil-and-paper tests to whole groups at a time. The interview failed to do this.

Using the paper-and-pencil tests, to which reference has been made, predictions can be made for 500 men with a total time expenditure of about 2 to 10 man-hours. Adding an individual interview to this prediction does not materially increase the efficiency of prediction, although it adds at least 750 man-hours to the time expended. Thus, although the interview shows promise of achieving useful levels of reliability and validity in the selection of pilots, its failure to add to the prediction obtainable by group techniques indicates that its excessive cost in time and money was proven to be not justifiable. Considering, for example, the fact that one Committee project[3] involved the examination, for the Civil Aeronautics Administration, of 67,000 applicants for flight training in 570 centers throughout the United States, it can

be seen that such findings have great practical and economic significance.

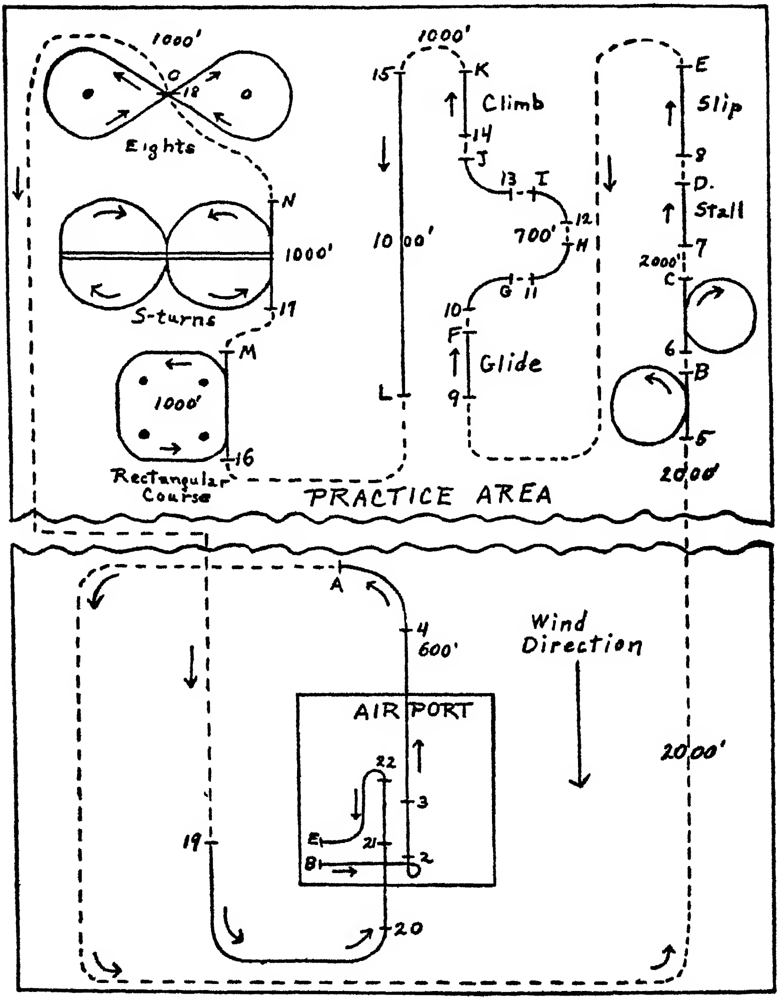
Individual physiological measures used in Committee investigations proved to be generally unpromising in the selection of pilots. For example, less than 1% of Naval Aviators revealed electroencephalograms which could be described as detrimental, indicating that whatever is measured by the electroencephalogram is, somehow or other, covered in the course of the established routine examinations and interviews given in the course of selecting Naval aviators. Results for the electrocardiogram and measures of aniseikonia were likewise negative in terms of practical considerations in the selection of aircraft pilots. Respiratory and cardiovascular measures proved to be largely unreliable, apparently giving, in the main, biological instants of organic functions rather than consistent measures of such functions to be relied upon as predictors of flight proficiency. These findings are of particular interest in relation to observations showing lack of consistency in medical examinations reported in studies for the Division of Research, Civil Aeronautics Administration, made independently of the Committee research program, by D. R. Brimhall and R. Franzen.[4, 5] Work continues in this area, particularly in the development of physiological measures with satisfactory reliability. An evaluation of 23 respiratory measures, for example, showed five measures to be sufficiently reliable for experimental comparison with flight success.[6]

Because of the basic importance of such standards of achievement in aviation research, early in the research program of the Committee, attention was centered upon the development of acceptable criteria of flight performance. Initial studies were, of course, centered on the adequacy of criteria actually in use. These studies showed that instructor ratings; scores on final examinations by inspectors; daily grades on flight performance and even, to some extent, the pass-fail criterion (at least for civilian pilots) were not of sufficient reliability or sufficiently discriminating to justify their use for research on the relative value of selection and training techniques. As a result, it was found necessary to proceed with the development of more objective, more reliable, and more discriminating measures of flight performance.

One of the first steps in the development of acceptable criteria was the preparation of *standard flights,* so that observation or recordings of flight performance could be made under essentially uniform conditions. A series of such flights, suitable for use at various levels of the

primary training course, was developed in research conducted at the University of Pennsylvania by the speaker with the assistance of Dr.

Exhibit 3
Standard Flight D
(for use at Boston Metropolitan Airport, Norwood, Mass. and Muller Field, Revere, Mass.)



November 20, 1941

Prepared by: Morris S. Viteles
Albert S. Thompson
Univ. of Penna.

A. S. Thompson of the Department of Psychology.[7] Such flights represent a "work sample" which calls for prescribed maneuvers to be carried through in a definite order and under stated conditions of wind velocity, etc. When standard flights are used it becomes possible to compare the flight proficiency of one individual with that of others, since all are required to perform the same maneuvers under essentially the same conditions.

EXHIBIT 3, Standard Flight D, describes a flight suitable for use in observing the performance of student pilots nearing the completion of the final stage in the C. P. T. primary program. It includes the more advanced maneuvers such as 360° turns, figure 8's, rectangular course, 180° approach to landing, etc. It is to be noted that the maneuvers included in these flights are of two types, *"critical maneuvers"* (indicated by solid lines), and *"transition maneuvers"* (indicated by dotted lines). Each transition maneuver is designed to put the plane into position for the succeeding critical maneuver. The transition maneuvers can be changed so as to adapt the flight to any level or type of civilian or military training program and to any airport.

With standardization of the situation under which the pilot's performance is evaluated, it became possible to take steps towards the improvement and objectification of criteria of achievement in learning to fly. The general character and reliability of direct observations of

EXHIBIT 4

CLIMBING TURN

CONTROL USE	Entry	Turn	Recovery
SIMULTANEOUS.....	☒	☐	☒
Successive............	☐	☒	☐
Slips.................	☒	☒	☐
Skids.................	☐	☐	☐
NEITHER...........	☐	☐	☒
Rudder Pressure:			
CORRECT..........	☐	☐	☒
Incorrect............	☒	☒	☐

PRECISION		
Bank:	CONSTANT....	☒
	Varies.........	—°
Speed....................		55 MPH
CORRECT Climbing Speed reading is.........		53 MPH
Speed is	CONSTANT....	☐
	Varies.....	3 MPH

flight performance, by instructors or inspectors actually in the plane with the students, were improved by providing specially designed forms calling for ratings on specific aspects of flight performance. A portion of one such form, *"The Ohio State Flight Inventory"* prepared by H. A. Edgerton and R. Y. Walker,[8] is illustrated in EXHIBIT 4.

Considerable work was done at Tulane University and at the University of Pennsylvania in analyzing the possibilities of commercial graphic instruments which provided a record of control movements and of changes in the attitude of the plane during flight.[9] Commercial recorders were found unsuitable, but the study led to the development of specifications for the construction of a new flight recorder which provides a graphic record of skids and slips; of changes in altitude; of variations in air speed; and of control movements associated with changes in plane attitude.

Perhaps the most significant development in the way of techniques for recording and evaluating flight performance has taken the form of a motion picture camera mounted in the plane to provide records for analysis on the ground. Problems of photographic recording were attacked independently at the University of Rochester and at the University of Pennsylvania.[10] While the Rochester project, with the cooperation of the Director of Research, CAA, proceeded immediately to the development of a concealed photographic unit, including an instrument panel and control movement indicator, the Pennsylvania group, in its earlier studies, photographed directly the plane instrument panel and the actual manipulation of controls by the pilot.

Present installations, adapted with the aid of the staff of the Institute of Aviation Psychology, University of Tennessee, provide for photography of an instrument panel containing flight instruments and a control movement recorder located in the baggage compartment of the plane. Both the photographic installation and methods of analyzing photographic records developed in research at the University of Pennsylvania have been widely used in obtaining objective criterion data in Committee research projects.

Graphic and photographic records and associated methods of analysis do not yield direct measures of certain aspects of pilot performance, such as observance of safety precautions, use of certain types of judgment, etc. They yield measures primarily of value in analyzing the skill displayed in the execution of maneuvers and are particularly useful in research where detailed and objective information on

the level of skill exhibited by the pilot is desired. Because of these objectives, graphic and photographic records provide basic data for determining the reliability of a single test flight, and in selecting, for pilot assessment, those aspects of flight performance which are relatively stable from flight to flight. It is also possible that graphic and photographic methods could be used for diagnosing specific faults of students who have difficulty in learning to fly, in much the same way as motion photographs are currently employed in the analysis of the faults made by members of football teams during actual play.

As the selection program has become stabilized, and improved criteria for the measurement of pilot performance have become available, it has been possible for the Committee on Selection and Training of Aircraft Pilots to center increasing attention upon the very important problem of improved methods for training pilots. Research in this area has, without question, placed the United States in the forefront of all nations in the matter of an experimental attack upon the problems of learning and teaching in aviation.

The Committee went about the job of research in pilot training in a very practical way. It started by finding out just what flight instructors actually did and said in teaching pilots. Discussions with both flight instructors and students showed that there were wide variations in instructional practices. However, nobody had had an opportunity to observe directly how the instructor went about the job of training pilots, especially in the primary and secondary phases of instruction, since observers could not be carried in the two-place planes used for such instruction. It was therefore found necessary, as a first step in research on training, to develop methods for listening in on conversations between an instructor and student during training flights and for recording such conversations. This was done by E. L. Kelly through the construction, at Purdue University, of ground-to-air transmission and recording apparatus which made available for analysis student pilot-instructor conversations.

Such an analysis made available many interesting items about the nature of instruction in the air. Frequently, the recordings were most revealing to the instructors who, when given an opportunity to listen to their conversations, were amazed to hear just what they had said. So, for example, until he heard his recorded conversation, one instructor refused to believe that he was telling students that the airplane steered just like a "sled," when the reverse is actually the case. Another be-

came aware, for the first time, why so many of his students were failing spins in the final flight examination when he heard his voice consistently go into a high pitch of hysteria as the student went into the spin and undertook to recover.

Qualitative observations of this type proved to be extremely helpful in revealing how the instructor taught and what students went through in learning to fly. Even more interesting were the findings of the systematic analyses of flight instruction recordings. One of the most significant of these was the revelation of great variation in vocabulary or terminology from instructor to instructor. In a study made of pre-solo dual instruction of four students by four instructors, it was found that 500 technical terms or phrases were used.[11] Many of these terms were peculiar to the instructor and were used without explanation to the student. Only one-seventh of the 500 terms were used by all four instructors. Over half of them were used by only *one* of the *four* instructors. Approximately one-third of the terms used were employed only once and by only one instructor. Such terms represent not only a private language of the individual instructor, but also are rarely used by that instructor.

These and other findings, in studies by E. L. Kelly and E. S. Ewart, led to the recognition of two very real needs: (a) for a set of short, simple, uniform, descriptive study sheets describing each maneuver to be studied by the student before receiving instruction in the maneuver; and (b) for a standardized patter for use by instructors during flight instruction. There was therefore prepared, for student use, a series of study sheets known as the *"Fundamentals of Basic Flight Maneuvers"* providing a description in the simplest and most straightforward fashion of the minimum essentials of information concerning each maneuver, which should be known by the student before attempting the maneuver. Sheets are bound loose-leaf to permit an instructor to hand out the sheet covering the particular maneuver which the student will take up in his next lesson, and which he is required to review before the next session of flight instruction. This is then again gone over on the ground before the student and instructor take to the air for flight instruction.[12]

For the instructor, there was provided a little bound volume of PATTER, including a standardized statement concerning the maneuver for use during flight instruction. The PATTER substitutes uniform descriptions of the maneuvers to be performed for the vary-

ing statements made to different students, when the instructor depended more or less upon the inspiration of the moment to decide how he would go about giving instructions on the maneuver. In passing, it is interesting to note that the idea of using PATTER was borrowed from the British, but that the PATTER books developed for use in training American student pilots were unlike those used by the British, in that they were based upon a factual analysis of flight instruction, to which reference has been made.

Perhaps the best cue to the value of these manuals, derived from the experimental studies of instructional techniques, is to be found in the fact that the Committee on Selection and Training of Aircraft Pilots was asked to supply personnel to help the Navy in preparing similar manuals for use in training Naval aviation cadets, and in the additional fact that the first order for such manuals for use by the Navy called for twenty thousand of each. The preparation of PATTER for the primary course was followed by the preparation of similar materials for the secondary course, and this material has been widely used in the instructor program of the War Training Service of the Civil Aeronautics Administration, as well as in modified form by the services.

Another practical outcome of Committee research in the area of training has been to focus attention upon the need of giving pilot instructors training in how to teach. In 1943, the Committee administered for the War Training Service of the Civil Aeronautics Administration two institutes at which *methods instructors* were given an intensive and practical course in training methods based on Committee research findings.[13] In addition to receiving instruction at the Institute, methods instructors were furnished with a "kit" of demonstration materials and samples of training aids which might prove effective in their teaching. A number of synthetic training devices were also added to aid in the instruction in the field. Both the Army and the Navy have also recently established special schools for instructors at which procedures similar to those embodied in the methods instructors' manual prepared by the Committee on Selection and Training of Aircraft Pilots are being used to put psychology to work in improving the training methods employed by pilot instructors.

Research on training continues. For current research, the Committee on Selection and Training of Aircraft Pilots has available a new instrument which is proving to be an effective aid in its investigations.

This is an air-borne model of a magnetic wire recorder developed at the Armour Research Foundation, with the cooperation of E. L. Kelly, as the result of the initiative and cooperation of the Committee on Selection and Training of Aircraft Pilots and of the Division of Research of the CAA. In this air-borne instrument, conversation between pilot and instructor is recorded on a steel wire only four-thousandth of an inch in diameter. With an instrument weighing only approximately 12 lbs., it is possible to record in the air a conversation lasting for one hour on a reel of wire only approximately four inches in diameter and one inch across.

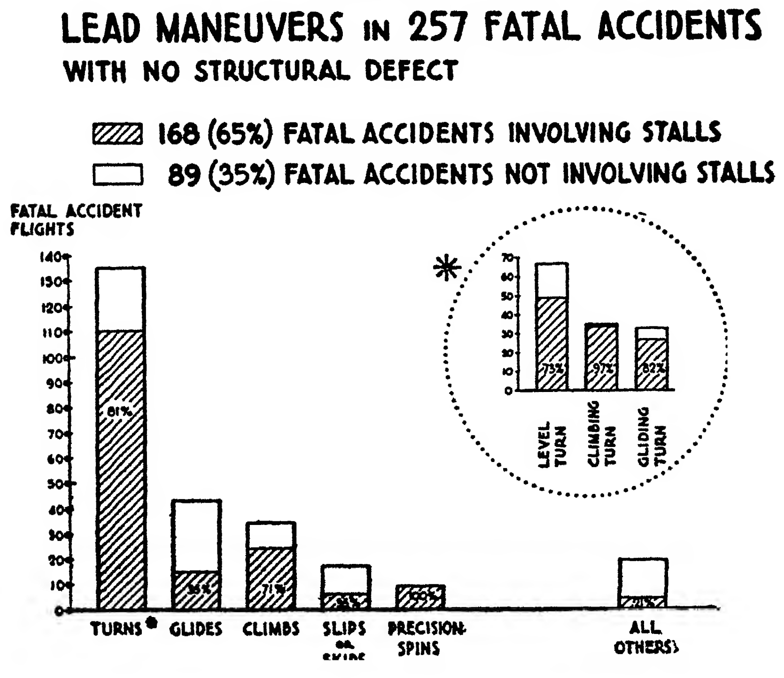
The air-borne model of the magnetic wire recorder has been used to great advantage in the current research program of the Committee on Selection and Training of Aircraft Pilots. One item in this research program involved a comparison of two groups of student pilots, one receiving instruction with the use of certain training aids and the other without the use of training aids. Photographic records of the performance of both groups during standard flights have been made, as a means of determining whether the use of the training aid makes a significant contribution to the improvement of flight proficiency. In another experiment, inspectors made check flights and recorded their observations verbally through the use of the magnetic wire recorders. Simultaneously, photographic records were made of the actual control movements of the pilot and of the attitudes assumed by the plane. In this manner, it becomes possible to determine items of plane and pilot performance which can, and those which cannot be accurately observed and reported upon by inspectors. In numerous other studies, the combination of wire recording of conversations and photographic recordings of pilot control movements and plane attitude furnishes the basis for an objective and factual analysis of what actually goes on in the plane, as a substitute for the arm chair rationalization and artificial laboratory investigations which were employed prior to the present war, in research on the human element in aviation.

As indicated earlier, Committee investigations have also been concerned with the problems of pilot maintenance and adjustment. In one such study, by D. Lewis, the effect of noise and vibration upon pilot performance has been studied. Considerable work has been done by C. Taylor and R. Franzen upon the evaluation of the Schneider Index and other standard tests of "exercise tolerance," and such investigations have suggested the possibility of substituting a more reliable

and effective measure of so-called "physical fitness," or of what might better be described as "exercise tolerance," for the existing measures.[14, 15] Considerable attention has been given to problems of motion sickness in the air in experiments by G. R. Wendt at Wesleyan University. These have led to the publication of a popular pamphlet entitled "How to Prevent Airsickness" which presents to the flight instructor and the student pilot a few simple rules which, there is reason to believe, can be effective in reducing the incidence of airsickness. The problem of tension has also been investigated in some detail.

In a major study,[16] the causes of civil aviation accidents, and the maneuvers most closely related to these accidents, are being investigated by D. R. Brimhall and R. Franzen. The extent to which fatal accidents are associated with stalls, particularly those growing out of turns at low altitudes, has suggested the necessity of important shifts of emphasis in the training program (EXHIBIT 5). A practical outcome of such research findings and conclusions would be a shift in emphasis

EXHIBIT 5



during training to the avoidance of and immediate recovery from the stall condition, possibly through extended practice in slow flying.

It is apparent from what has been said that the research interests of the Committee on Selection and Training of Aircraft Pilots have been wide and varied. They have been directed toward civilian as well as military flying. They have led to the development of tools and techniques which have received wide acceptance both in civilian pilot training and by the military services. Nevertheless, the surface has only been scratched, and there is every reason to believe that continued and extended investigations are needed for the solution of many of the real problems experienced when human beings undertake to fly bigger, faster, and different planes.

To this end, the Committee, in cooperation with the Civil Aeronautics Administration and the Tennessee Bureau of Aeronautics, has already taken steps toward the assurance of postwar research through the establishment of an Institute of Aviation Psychology at the University of Tennessee. A similar center is currently being set up at Ohio State University where the relationship between visual efficiency and flight performance will be carefully determined. In this way, the work of the Committee on Selection and Training of Aircraft Pilots, supported by the Civil Aeronautics Administration, is serving to reduce the danger of cessation of research in aviation psychology which occurred at the end of World War I.

BIBLIOGRAPHY

1. **McFarland, R. A., & R. Franzen.**
   1944 (Nov.). CAA Division of Research, Report No. 38. Washington, D. C.
2. **Dunlap, J. W., & M. J. Wantman.**
   1944 (Aug.). An investigation of the interview as a technique for selecting aircraft pilots. CAA Airman Development Division, Report No. 33. Washington, D. C.
3. **NRC Committee on Selection and Training of Aircraft Pilots.**
   1944 (Nov.). CAA Division of Research, Report No. 39. Washington, D. C.
4. **Franzen, R., & D. R. Brimhall.**
   1942 (Feb.). Problems of consistency arising from CAA medical examinations. Division of Research, Civil Aeronautics Administration. Washington, D. C.
5. **Franzen, R., & D. R. Brimhall.**
   1942 (July). Analysis of physical defects found by the armed services in pilots certified to be without disqualifying defect by Civil Pilot Training examination. Division of Research, Civil Aeronautics Administration. Washington, D. C.

6. **Franzen, R., & L. Blaine**

1944 (Jan.). CAA Division of Research, Report No. **25**. Washington, D. C.

7. **Viteles, M. S., & A. S. Thompson.**

1943 (May). The use of standard flights and motion photography in the analysis of aircraft pilot performance. CAA Division of Research, Report No. **15**, Washington, D. C.

8. **Edgerton, H. A., & R. Y. Walker.**

—— History and development of the Ohio State Flight Inventory, Part I: Early versions and basic research. (A final report in preparation for publication in the CAA Technical Series.) Washington, D. C.

9. **Viteles, M. S., & O. Backstrom, Jr.**

1943 (Nov.). An analysis of graphic records of pilot performance obtained by means of the R-S ride recorder. Part. I. CAA Division of Research, Report No. **23**. Washington, D, C.

10. **Viteles, M. S., & A. S. Thompson.**

1944 (July). CAA Division of Research, Report No. **31**. Washington, D. C.

11. **Kelly, E. L.**

1943 (Oct.). The flight instructor's vocabulary. CAA Division of Research, Report No. **22**. Washington, D. C.

12. **Kelly, E. L., & E. S. Ewart.**

1942 (Dec.). CAA Division of Research, Report No. **6**. Washington, D. C.

13. **Viteles, M. S., R. Y. Walker, E. S. Ewart, H. S. Odbert, R. C. Rogers, A. S. Thompson, & D. R. Brimhall.**

1943 (Sept.). CAA Division of Research, Report No. **20**. Washington, D. C.

14. **Taylor, C.**

1941. Studies in physical fitness. Unpublished final report.

15. **Franzen, R.**

1943 (Aug.). Notes I, II, III. Correlations with treadmill scores and intercorrelations of functions of cubic heart rate trends during exercise. Progress report. Washington, D. C.

16. **Franzen, R.**

1944 (July). Report on a study of fatal airplane accidents. In: Annual Meeting of Committee on Selection and Training of Aircraft Pilots. National Research Council Division of Anthropology and Psychology. Washington, D. C.

SECTIONS OF BIOLOGY AND PHYSICS AND CHEMISTRY

MAY 18 AND 19, 1945

Conference on *"Blood Grouping."*

The Sections of Biology and Physics and Chemistry held a Conference on "Blood Grouping," as the seventh in the series for the Academic Year 1944–1945. Doctor William C. Boyd, Boston University, School of Medicine, Boston, Massachusetts, was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

"Introduction to the Conference," by William C. Boyd.

"Isolation and Purification of Blood Group A and B Substances; Their Use in Conditioning Universal Donor Blood, in Neutralizing Anti-Rh Sera, and in the Production of Potent Grouping Sera," by Ernest Witebsky, University of Buffalo, Medical School, Buffalo, New York.

"Method for the Production of Anti-A, Anti-B and Anti-Rh Isoagglutinin Reagents: A. Choice of Plasma. B. Fractionation Process. C. Absorption and Neutralization Process. D. Standardization of Product," by J. L. Oncley, M. Melin, J. W. Cameron, D. A. Richert, L. K. Diamond, Harvard Medical School, Boston, Massachusetts.

"Recent Advances in the Immuno-Chemistry of Isohemagglutinins," by Louis Pillemer, Western Reserve University, Institute of Pathology, Cleveland, Ohio.

"Procurement and Preparation of Blood Grouping Serum," by Captain John Elliott, Army Medical School, Washington, D. C.

"The Assay of Blood Grouping Sera; Variation in Reactivity of Cells of Different Individuals Belonging to Groups A and AB," by William C. Boyd, Boston University, School of Medicine, Boston, Massachusetts.

"Genetic and Constitutional Causes of Fetal and Neonatal Morbidity," by Philip Levine, Ortho Research Foundation, Linden, New Jersey.

"The Rh Series of Allelic Genes, with Special Reference to Nomenclature," by A. S. Wiener and Eve B. Sonn, Medical Examiner's Office, New York, N. Y.

ANNOUNCEMENT OF ADDITIONAL PUBLICATIONS FOR 1945[1]

The following publications, in addition to those announced in the April number of the TRANSACTIONS, will be issued by the Academy during the current year. Members of the Academy who desire to receive these will kindly request the Executive Secretary to send them, and they will be mailed, free of charge, as they are ready for distribution, except as qualified in the footnotes:[2]

ANNALS

7. "Non-Projective Personality Tests." Papers delivered at the conference by this title. (Approximately 75 pages.)

8. "Lymph." Papers delivered at the conference by this title. (Approximately 150–200 pages.)

9. "Blood Grouping." Papers delivered at the conference by this title. (Approximately 125 pages.)

10. "The Golgi Apparatus—A Modern Interpretation," by Leonard G. Worley. (Approximately 40 pages.)

11. "Brain and Body Weight in Man: Its Antecedents in Growth and Evolution," by Earl W. Count. (Approximately 110 pages.)

[1] Notice of any additional papers to be added to this list will be sent to Members as they are approved for publication

[2] Active, Sustaining, Life, and Honorary Members may receive, upon request, a copy of all current numbers of the Annals

Associate and Student Members are entitled to receive one complete monograph, or up to 150 pages of smaller papers.

NEW MEMBERS

Elected May 15, 1945

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Crowninshield, Vincent F., M.A., Psychology. Assistant to Personnel Director, Johnson and Johnson, New Brunswick, New Jersey.

Friedmann, Jechiel Moses, M.D., Psychology, Biology. Practicing Neuropsychiatrist, New York, N. Y.

Gardner, William Howlett, Ph.D., Chemist, New Products Division, National Aniline Division, Allied Chemical and Dye Corporation, New York, N. Y.

Haring, Robert C., Ph.D., Industrial Organic Chemistry. Research Chemist, National Aniline Division, Allied Chemical and Dye Corporation, New York, N. Y.

Hill, Ella C., B.S., Anthropology, Natural Sciences, Docentry. American Museum of Natural History, New York, N. Y.

Lederberg, Joshua, A.B., Cell Physiology. Student, Columbia University Medical School, New York, N. Y.

Meister, Alton, B.S., Medicine and Biochemistry. Interne in Medicine, New York Hospital, New York, N. Y.

Seigle, L. W., Ph.D., Organic Chemistry. Research Chemist, New Products Division, National Aniline Division, Allied Chemical and Dye Corporation, New York, N Y.

Watts, Nellie Perry, Ph.D., Drugs. Research Associate, Department of Therapeutics, New York University, New York, N. Y.

ASSOCIATE MEMBERS

Bartner, Elliot, B.S., Chemistry. Research Assistant, Rutgers University, New Brunswick, New Jersey.

Howard, Gerald V., B.A., Fisheries Conservation. Junior Biologist, International Pacific Salmon Fisheries Commission, New Westminster, British Columbia, Canada.

TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 8 NOVEMBER, 1945 No. 1

SECTION OF GEOLOGY AND MINERALOGY

OCTOBER 1, 1945

DOCTOR HORACE G. RICHARDS, Associate Curator of Geology and Paleontology, Academy of Natural Sciences, Philadelphia, Pa.: *The Subsurface Stratigraphy of the Atlantic Coastal Plain.* (The lecture was illustrated by lantern slides.)

Since the beginning of the war, there has been a considerable increase in the number of wells drilled along the Atlantic Coastal Plain. This increase was caused by two factors: (1) the construction of many army camps, naval bases and industrial plants and the consequent need for additional supplies of ground water; and (2) the oil shortage and the increased search for new supplies.

With the aid of a grant from the Penrose Bequest of the Geological Society of America, the writer has undertaken a study of the samples from many of these wells, and has endeavored to correlate the subsurface formations.[1] While emphasis has been placed on the macrofossils, especially mollusks, all other available evidence has been considered, including the microfossils, lithology and a study of the literature. The region under consideration extends from New Jersey through Georgia

[1] **Richards, H. G.** Subsurface stratigraphy of Atlantic Coastal Plain between New Jersey and Georgia. Bull. Amer. Assoc. Petroleum Geol. **29:** 885–955. 1945.

TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 8, No. 1, November, 1945

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Editor: Roy Waldo Miner.

Executive Secretary: Eunice Thomas Miner.

Entered as second-class matter December 2, 1938, at the post office at Lancaster, Pa., under the act of August 24, 1912.

and, so far, samples from about 200 wells have been studied in more or less detail. The project is being continued.

So far, no productive oil wells have been drilled along the East Coast, north of Florida. The various dry holes drilled in this area have yielded considerable information on the subsurface and, while the evidence can not be said to be encouraging, nevertheless, the East Coast can not yet be written off as impossible.

A few of the more significant oil tests along the East Coast are listed here:

Approximate Location	*Company*	*Basement*	*Total Depth*	*Date*
Jacksons Mills, Ocean Co., N. J.	N. J. Oil & Gas Fields Co.; W. & K. Oil Co.	1336	5022	1921
Bridgeville, Sussex Co., Del.	Sussex Oil Co.; Sun Oil Co.	—	3010	1935
Salisbury, Wicomico Co., Md.	Ohio Oil Co.	5529	5563	1944–5
Berlin, Worcester Co., Md.	Socony Vacuum Oil Co.	7150	7168	1945
Meadows, Prince George Co., Md.	....	1522?	1522	1918
Matthews, Matthews Co., Va.	Elkins Oil Co.	2307	2325	1929
Havelock, Craven Co., N.C.	Great Lakes Drilling Co.	2318	2318	1927
Morehead City, Cartaret Co., N.C.	Coastal Plains Oil Co. (Karsten)	4021	4044	1945
Cape Hatteras, Dare Co., N.C.	Standard Oil of N. J.	now drilling		
Conway, Horry Co., S.C.	Pioneer Oil Co.	1400	1429	1939
Summerville, Dorchester Co., S.C.	....	2450?	2450	1921?
Savannah, Chatham Co., Ga.	Savannah Oil Co.	—	2130	1920
Offerman, Pierce Co., Ga.	Pan American	4355	4375	1938
Offerman, Pierce Co., Ga.	Donald Clark	4335	4355	1939
Albany, Daughtery Co., Ga.	J. R. Sealy, No. 1	—	5013	1942
Cedar Springs, Early Co., Ga.	Mont Warren *et al*-Chandler	—	7240	1943
Camilla, Matchell Co., Ga.	Stanolind Oil & Gas Co.	7474	7488	1944
McKinnon, Wayne Co., Ga.	California Co.	4575	4626	1944
Pearson, Atkinson Co., Ga.	Sun Oil Co.	4220	4283	1944

For more complete list of oil tests see Richards (*op. cit.*). The tests have been distributed along the Coastal Plain as follows:

New Jersey	9
Delaware	3
Maryland	5
Virginia	1
North Carolina	4
South Carolina	6
Georgia	30

Among the more significant results, may be mentioned the following:

1. Shell fragments and Foraminifera were found in wells at Asbury

Park, N. J., Fort Dix, N. J., and Freehold, N. J., in the Englishtown formation, from which no fossils have hitherto been reported.

2. Shell fragments and/or Foraminifera have been found in the Raritan formation at Fort Dix, N. J., Clementon, N. J., Bridgeville, Del., and Salisbury, Md. This formation is correlated with the Tuscaloosa.

3. The presence of Lower Cretaceous non-marine sediments is suggested from a well at Salem, N. J.

4. Jackson Eocene fossils were found in wells at Atlantic City, N. J., Brandywine Lighthouse, N. J., Bridgeville, Del., and Salisbury, Md. These mark the northernmost extension of the late Eocene.

5. Midway (Paleocene) fossils are reported from the subsurface in New Jersey (lower Hornerstown), Delaware, Maryland and Virginia.

6. The well at Salisbury, Md. showed a remarkable thickness of non-marine Lower Cretaceous (3000 feet). Preliminary studies show an even greater thickness at Berlin, Md.

7. At both Salisbury and Berlin, the Lower Cretaceous was underlain by rocks of probable Triassic age. Basement was struck at Salisbury at 5529 feet and at Berlin at 7150 feet.

8. Although the Eocene is very thin in Virginia, south of the James River, Lower (Midway), Middle (Pamunkey) and Upper (Jackson) fossils have been recognized.

9. Marine fossils assigned to the Tuscaloosa (Upper Cretaceous) have been found in wells at Norfolk, Va., Drivers, Va., and Franklin, Va. This formation does not outcrop in Virginia.

10. Middle Eocene (Pamunkey) fossils have been found in a well at Williamston, North Carolina. This is the first record of older Eocene from this State.

11. The Tuscaloosa, which is non-marine in outcrops in the Carolinas, contains marine fossils at Havelock, N. C., Morehead City, N. C., Conway, S. C., and Parris Island, S. C.

12. The well at Havelock, N. C., encountered granite at 2319 feet, while the Karsten well, near Morehead City, only 17 miles away, reached granite at 4021 feet. Havelock may represent the top of an ancient hill, while Morehead City may be the base. A similar hill in the basement is noted at Fountain, N. C., where the granite is exposed at the surface, whereas a mile or so to the east and west, it is buried 200 feet or more.

13. The Triassic has been reported from wells at Florence, S. C., and Summerville, S. C. However, no samples are available for study.

14. An exceptional thickness of marine Tuscaloosa is noted in the Sealy No. 1 well, near Albany, Ga. (2313 feet).

Descriptions and notes on the rare or new species of macrofossils are being prepared and will be submitted to the Journal of Paleontology.

SECTION OF BIOLOGY

October 8, 1945

Doctor M. J. Kopac, Assistant Professor of Biology, Washington Square College of Arts and Science, New York University, New York, N. Y.: *Cellular Mechanisms in Chemotherapy.* (This lecture was illustrated by lantern slides.)

The selective destruction of neoplastic cells by chemical compounds is more difficult than the control of invading microorganisms. One may expect sufficient biochemical differences between the host and parasite to make possible the destruction of the microorganisms without damaging host tissues. On the other hand, the differences between neoplastic cells and their normal ancestors are far more subtle and, in many cases, so slight as to make difficult the destruction of one cell type without seriously damaging the normal precursors.

Differences between neoplastic cells and their normal ancestors do exist. For example, Chambers, Cameron and Kopac[1] showed that lymphoid cells obtained from malignant tissues were more susceptible, in tissue cultures, to destruction by tetramethyl-o-phenylenediamine than were the lymphocytes obtained from normal lymph nodes. Cameron, Kensler and Chambers[2] demonstrated differences in reactivity to certain chemical compounds between other neoplastic cells and their normal prototypes.

The methods used in testing chemical compounds for oncolytic activity generally fall into two groups: (1) enteral or parenteral administration of the compound to animals with spontaneous or transplantable neoplasms, and (2) exposure of established tissue culture growths containing mixed populations of neoplastic and non-neoplastic cells to solutions of the compound. Hundreds of organic and inorganic compounds have been tested for oncolytic activity by many investigators, with the majority using the first method. The second method, however, gives more specific data on the action of a compound on the neoplastic cells.

Roffo[3] reported that hydrolysates of striated muscle inhibited the growth of certain rat and mouse neoplasms *in vivo* and *in vitro*. Boyland[4] localized the growth-inhibitory substances in the trichloracetic acid-soluble, phosphotungstic acid-insoluble fraction of mineral acid

extracts of muscle tissues. Such fractions would contain the amines, guanidines, and related nitrogenous bases.

The problem of neoplastic chemotherapy is considered from three points of view: (1) the nature of cytological changes produced by the action of those compounds that selectively destroy neoplastic cells without seriously damaging normal cells; (2) the determination of the action on secretory activity of kidney tubules, in tissue culture, of those compounds that destroy neoplastic cells; and (3) the determination of various physico-chemical properties of those compounds that destroy neoplastic cells and comparison with the properties of related compounds that do not destroy neoplastic cells.

A variety of organic compounds including ethylamines, diphenylethylamines, isothioureas, thioureas, aromatic diguanidines, and aromatic diamidines were tested on kidney tubules, in tissue culture.[5] The most interesting compounds, on the basis of their chemical structure and of their action on kidney tubules and on neoplastic cells, are the aromatic diamidines.[5, 6] In the kidney tubule preparations,[7] the action of the compounds can be tested simultaneously on secreting and non-secreting epithelia, connective tissue cells, wandering cells, and others. For example, the compound S-methylisothiourea, at certain concentrations, destroys all non-secreting structures, while the secreting structures continue to function almost normally. By comparison, N-methylthiourea, at the same concentrations, is non-toxic to both secreting and non-secreting components of the tissue culture preparation.[5]

The action of selected compounds on protein molecules, as well as on nucleoprotein systems, is being determined by surface-chemical methods. For the study of protein denaturation, the procedure is to expose protein molecules to interfacial forces in the presence of a given compound, at concentrations of the same order as those used in biological tests. Any incipient denaturating action of the agent will produce a significant increase in surface-denaturation of the protein and this can be measured by the surface-chemical methods employed (Kopac[8]).

Of the 6 aromatic diamidines tested, stilbamidine diisethionate, at concentrations not affecting normal tissues, showed a specific destructive action on the neoplastic cells of the rat mammary adenocarcinoma, R2426 (Eisen[9]), and of the transplantable lymphosarcoma of the rat (Murphy and Sturm[10]). The other diamidines, including phenamidine,

propamidine, and pentamidine, killed all normal and neoplastic cells at the higher concentrations and failed to destroy any of the cells at lower concentrations.[6]

Several effects were observed in the nuclei of the adenocarcinoma cells treated with stilbamidine. These changes include: irregularity in nuclear size and shape, increase in nuclear granules and blebs, and pyknosis. Stowell[11] reported similar changes in the nuclei of the R2426-tumor cells following treatment, *in vivo*, with 4000 roentgens of 200-kv x-radiation. Furthermore, stilbamidine blocks mitosis in neoplastic cells at concentrations that have no effect on the mitotic processes of normal or non-neoplastic cells.

Stilbamidine enhances the surface-denaturation of serum albumin to a considerable degree. The other diamidines do so slightly, or not at all. These results indicate that stilbamidine, more so than the other diamidines, can weaken or rupture certain structural linkages, presumably, the side chains held together by hydrogen bonds in the albumin molecule. These weakened linkages, when augmented by interfacial forces, produce a significant increase in surface denaturation.

The micro-surface-chemical methods[8]—simultaneous exposure of protein molecules to surface forces and the diamidine—therefore permit the measurement of incipient denaturation in protein molecules as produced by low concentrations of a denaturating agent.

A recent development[12] involves the action of stilbamidine, and other diamidines, on protamine-ribonucleates which represent simple models of the nucleoproteins. Stilbamidine diisethionate, at 0.002 M or less, produces an unfolding (denaturation) of the protamine molecule, as indicated by surface-chemical methods. Stilbamidine, at similar concentration levels, dissociates protamine-ribonucleate to release the protamine and simultaneously traps the ribonucleate by the formation of an insoluble stilbamidine-ribonucleate complex.

The surface-chemical methods are so employed that the degree of protamine-ribonucleate dissociation can be estimated by measuring the interfacial adsorption of liberated protamine at appropriate oil-water interfaces. The reactions include:

$$\underline{\text{Protamine-ribonucleate}} + \text{n } \underline{\text{Stilbamidine}} \rightarrow \underline{\text{Stilbamidine}_{\text{n}}\text{-ribonucleate}}$$

[insoluble complex]

partial denaturation

$$+ \text{ [Protamine]} \longrightarrow \text{Increased interfacial adsorption.}$$

The dissociation of protamine-ribonucleate results from the partial denaturation of protamine and possibly from the denaturation (alteration in configuration) of ribonucleic acid. Nucleoproteins, therefore, can be destroyed by low concentrations of stilbamidine.

Propamidine diisethionate, on the other hand, does not dissociate protamine-ribonucleate, nor does it produce any significant unfolding (denaturation) of the protamine molecule. However, it does form complexes with Na-ribonucleate, although these may be slightly more soluble than the stilbamidine-ribonucleates.

In all probability, the property of a compound to dissociate protamine-ribonucleate is dependent on its ability to denature the protein moiety.

The first toxic, intracellular action of stilbamidine appears to be the dissociation of protamine-ribonucleate complexes. Stilbamidine thereby releases partially denatured protamine and simultaneously binds the nucleic acids. Destruction of certain nucleoproteins, *per se,* may be sufficient to kill the cell.

The second toxic reaction follows the liberated and partially denatured protamine molecules. These protamine molecules, situated intracellularly, can now react by salt linkages with a variety of phosphorylated compounds including: adenosine di- and tri-phosphates, diphosphopyridine nucleotide, phosphocreatine, diphosphothiamine, and the phospholipids. Many of these substances may be temporarily or permanently inactivated depending on the nature of the complexes, *e.g.*, solubilities and dissociation constants. Although stilbamidine can interact directly with the phosphorylated compounds, there is some evidence to indicate that the resulting complexes are less stable and more soluble than are those formed with ribonucleic acids.

Propamidine diisethionate does not dissociate protamine-ribonucleates and, accordingly, this substance cannot destroy nucleoproteins nor can it release partially denatured protamine. Propamidine may, however, inactivate some of the phosphorylated compounds by forming complexes with them. In this way, the toxic nature of propamidine becomes evident, although this agent lacks the specificity shown by stilbamidine.

Differences in nucleoprotein content, between neoplastic cells and their normal ancestors, both quantitative and qualitative, have been

described by several investigators, especially by Stowell,[13] and Stowell and Cooper.[14]

One may likewise expect considerable variation in physico-chemical properties of these nucleoproteins. Accordingly, it should be possible to dissociate certain nucleoproteins without damaging others. Such a task would require the synthesis of compounds possessing the necessary specificity to denature the protein moiety and thereby dissociate the desired nucleoprotein components of a given cell.

The specificity shown by stilbamidine in destroying certain neoplastic cells may be interpreted in two ways: (1) that this diamidine is sufficiently specific to dissociate those nucleoproteins essential to the neoplastic cells, or (2) that neoplastic cells contain nucleoproteins more readily dissociated by stilbamidine, than are those present in normal cells.[12]

Propamidine, on the other hand, does not denature protamine and thereby lacks the ability to dissociate similar nucleoproteins. One should recall that, with stilbamidine, the nucleoprotein complexes can be broken, while stilbamidine itself becomes bound by the liberated nucleic acids. The released protamine, in turn, provides the additional toxic factor by binding essential phosphorylated and other acidic metabolic intermediates. Propamidine, instead, reacts directly with the acidic substances and, if its intracellular concentration becomes high enough, it functions as a general protoplasmic poison.

The key to specificity, therefore, lies in the dissociation of certain nucleoproteins. Not only can the latter be destroyed by appropriate substances but the activated or denatured protamine molecules, the dissociation products, are toxic.

It is hoped that the various data outlined herein may provide the necessary physico-chemical methods for evaluating the potential chemotherapeutic effectiveness of new compounds as they become available. Several of these are now being synthesized.

It seems clear, at present, that any compound, to be capable of destroying neoplastic cells selectively, must first have the property of dissociating certain key nucleoproteins. The dissociating potency of such compounds should be of the same order as that demonstrated by stilbamidine on simple protamine-ribonucleates.[12]

LITERATURE CITED

1. **Chambers, R., G. Cameron & M. J. Kopac**
   1943. Neoplasm Studies. XI. The effects in tissue culture of N, N, N[1], N[1], tetramethyl-o-phenylenediamine and other compounds on malignant lymph nodes. Cancer Research **3**: 293-295.
2. **Cameron, G., C. J. Kensler & R. Chambers**
   1944. The action of heptanal sodium bisulfite methyl salicylate and of 2, 4, 6-trimethyl-pyridine on tissue cultures of human and mouse carcinoma and rat lymphosarcoma. Cancer Research **4**: 495–501.
3. **Roffo, A. H.**
   1938. The action of a hydrolysate of striped muscle on malignant tumours. Lancet **235**: 184–187.
4. **Boyland, E.**
   1941. Experiments on the chemotherapy of cancer. 5. The effect of muscle extract and aliphatic bases. Biochem. J. **35**: 1283–1288.
5. **Kopac, M. J., G. Cameron & R. Chambers**
   1945. The action of aromatic diamidines and other compounds on kidney tubules in tissue culture. (To be published.)
6. **Kopac, M. J., G. Cameron & R. Chambers**
   1945. The action of aromatic diamidines on tissue cultures of rat and mouse neoplasms. (To be published.)
7. **Chambers, R., & R. T. Kempton**
   1933. Indications of function of the chick mesonephros in tissue culture, with phenol red. J. Cell. Comp. Physiol. **3**: 131–167.
8. **Kopac, M. J.**
   1943. Micrurgical application of surface chemistry to the study of living cells 26–71. Micrurgical and Germ-Free Methods. J. A. Reyniers, ed. Springfield, Ill.
9. **Eisen, M. J.**
   1940. Transplantable carcinoma of the rat breast. Amer. J. Cancer **39**: 36–44.
10. **Murphy, J. B., & E. Sturm**
    1941. The transmission of an induced lymphatic leukemia and lymphosarcoma in the rat. Cancer Research **1**: 379–383.
11. **Stowell, R. E.**
    1945. The effects of roentgen radiation on the thymonucleic acid content of transplantable mammary carcinomas. Cancer Research **5**: 169–178.
12. **Kopac, M. J.**
    1945. Some cellular and surface-chemical aspects of tumor chemotherapy. (To be published in Gibson Island Conference on Cancer Monograph.)
13. **Stowell, R. E.**
    1945. Thymonucleic acid in tumors. Cancer Research **5**: 283–294.
14. **Stowell, R. E., & Z. K. Cooper**
    1945. The relative thymonucleic acid content of human normal epidermis, hyperplastic epidermis, and epidermoid carcinomas. Cancer Research **5**: 295–301.

SECTION OF PSYCHOLOGY

OCTOBER 15, 1945

DOCTOR ROBERT A. BROTEMARKLE, Director of Psychological Laboratory and Clinic, University of Pennsylvania, Philadelphia, Pa.: *Problems in the Description of Personality. I. Sociability or "Extroversion-Introversion."* (The lecture was illustrated by lantern slides.)

The long history of man's attempts at the description of personality is well-known. In this paper, some problems in the description of personality are presented, which have been high-lighted anew for us by the most recent findings in the excellent studies by Raymond B. Cattell in his article on the "Principal Trait Clusters for Describing Personality."[1] In the review of his findings Cattell remarks: "In closing, it is relevant to point to the possibilities by some entirely free speculation. Once the data are preserved in the above index system against the sclerosis of premature verbal coinage and the insidious assault of implicit interpretation, freer discussion may safely be indulged in 'out of court.' One can note that a majority of the clusters established by correlation procedures seem to be actually well known syndromes of the clinic and the laboratory."

It is my desire to investigate with you some of the problems to be observed in these syndrome relationships, which prompts me to run the risk of considerable "implicit interpretation." However, since we cannot presume upon undue familiarity with the more recent and extensive findings of Cattell, I shall of necessity have to place before you something of the entire picture of his work, before turning to my study of the particular area of our discussion.

Cattell, in "The Description of Personality: Basic Traits Resolved into Clusters,"[2] adopted an approach which must be satisfactory to most of us when he states, "The position we shall adopt is a very direct one, verging on a pragmatic philosophy, and making only the one assumption that all aspects of human personality which are, or have been, of importance, interest, or utility have already become recorded in the substance of language."

[1] Psych. Bull. 42 (3): 129–161. 1945.
[2] J. Abnormal & Soc. Psych. 38 (4): 476–506. 1943.

Proceeding to the selection of a "categorized trait list," based upon the study of previous collected lists and certain known syndrome elements, Cattell listed 171 dichotomous or opposite pairs of traits in his "Final Population of the Trait Sphere."[3] From these traits, his extensive correlation studies resolved a series of "clusters" falling under certain "sectors" in the total personality "sphere." It should be noted that each trait-pair includes a number of trait terms which some might consider synonymous, or as giving expression to modifications or variations of the trait dichotomy.

At this point, it is important for us to have before us the briefly described "clusters" in their relationship to the "sectors" and the total "sphere," and likewise the commonly known syndrome or experimentally determined patterns related thereto.

In presenting Cattell's "Sphere of Personality,"[2] let us assume that each of us is standing in the center of the "sphere" and from this point of vision we will have coming to us the lines representing the various "sectors," "clusters" and trait-pairs, and we shall find innumerable lines of reference or relationship passing through us as they establish the numerous cross-relationships between the various "clusters."

THE SPHERE OF PERSONALITY

By Raymond B. Cattell

Region A

Character, Personality Integration	
vs	
Moral Character Defect, Neurosis, Psychosis	
SECTOR AA	
Fineness of Character	
vs	
Moral Defect, Non-persistence	
Cluster AA1	
Integrity, Altruism	"psychopathic personality"
vs	(constitutional character) RAB*
Dishonesty, Undependability	
AA2	
Conscientious Effort	(mental character)
vs	
Quitting, Incoherence	
SECTOR AB	
Realism, Emotional Integration	
vs	
Neuroticism, Evasion, Infantilism	

* This, and the succeeding "interpretative statements are added by the author of this article. [Ed.]

CLUSTER AB1 Realism, Reliability vs Neuroticism, Changeability	"pre-psychotic simple schizophrenia"
CLUSTER AB2 Practicalness, Determination vs Daydreaming, Evasiveness	"pre-psychotic simple schizophrenia"
CLUSTER AB3 Neuroticism, Self-deception, Emotional Intemperateness vs	"general neuroticism"
CLUSTER AB4 Infantile, Demanding, Self-centeredness vs Emotional Maturity, Frustration Tolerance	"conversion hysteria"
SECTOR AC Balance, Frankness, Optimism vs Melancholy, Agitation	
CLUSTER AC1 Agitation, Melancholy, Obstinacy vs Placidity, Social Interest	"constitutional agitated melancholia"
CLUSTER AC2 Balance, Frankness, Sportsmanship vs Pessimism, Secretiveness, Inordinateness	"pre-psychotic catatonic schizophrenia"
SECTOR B Intelligence, Disciplined Mind, Independence vs Foolish, Undependable, Unreflectiveness	
CLUSTER B1 Emotional Maturity, Clarity of Mind vs Infantilism, Dependence	"emotional maturity" (emotional toned)
CLUSTER B2 Gentlemanly, Disciplined, Thoughtfulness vs Extraverted, Foolish, Lack of Will	(social toned)
CLUSTER B3 Creativity, Self-determination, Intelligence vs Narrowness of Interests, Fogginess	(volitional toned)
CLUSTER B4 Intelligence, Penetration, General Talent vs Lack of "g"	"g"

Region C

Self-assertion, Venturesomeness, Clamourousness vs General inhibition, Modesty, Timidity	
SECTOR CA	
Egoism, Assertion, Stubbornness vs Modesty, Self-effacement, Adaptability	
Cluster CA1	
Crude social assertion, Exhibitionism	"inferiority over-compensation"
vs Modesty, Obedience to Authority	
Cluster CA2	
Stubbornness, Pugnacity, Clamourousness	"assertive-submissive"
vs Tolerance, Self-effacement	
Cluster CA3	
Rigidity, Despotism, Egotism vs	
Adaptability, Friendliness, Tactfulness	"assertive-submissive"
Cluster CA4	
Shrewd, Dictatorialness	"assertive-submissive"
vs Naive, Unassertiveness	
Cluster CA5	
Assertion, Rivalry, Conceit	"assertive-submissive"
vs Modesty, Unassumingness	
Cluster CA6	
Eager, Self-assertion	"assertive-submissive"
vs Lack of Ambition	
SECTOR CB	
Boldness, Independence, Toughness vs Timidity, Inhibition, Sensitivity	
Cluster CB1	
Energy, Boldness, Spiritedness	"somatotonic"
vs Apathy, Timidity, Langour	
Cluster CB2	
Independence, Cleverness, Confidence	"somatotonic"
vs Timidity, Dependence, Languidness	
Cluster CB3	
Lack of restraint, Adventurousness	"general inhibition"
vs General inhibition, Fearlessness	

Cluster CB4
Poised sociability, Inertia, Toughness
vs
Introspectiveness, Sensitivity, Haste
"hyper-thyroid hypersensitivity"

Cluster CB5
Smartness, Assertiveness, Independence
vs
Unsophistication, Submissiveness, Reverence

SECTOR D
Sociability
vs
Timidity, Hostility, Gloominess

Cluster D1
Sociability, Adventurousness, Heartiness
vs
Shyness, Timidity, Reserve
"extrovert-introvert" (general sociability)

Cluster D2
Sociability, Sentimentalism, Warmth
vs
Independence, Hostility, Aloofness
"extrovert-introvert" (general social interest)

Cluster D3
Interest in group life, Liking to participate
vs
Self-sufficiency
"extrovert-introvert" (social group interest)

Cluster D4
Personal attractiveness, Sociability, Pleasure seeking, Frivolity
vs
Earnestness, Asceticism, Mirthlessness
"extrovert-introvert" (socio-personal reactions)

Cluster D5
Cheerful, Enthusiastic, Witty
vs
Cold-hearted, Sour, Mirthless
"extrovert-introvert" (socio-evaluation reactions)

SECTOR E
General emotionality, High-strungness, Instability
vs
Placidity, Deliberateness, Reserve

Cluster E1
High-strungness, Impulsiveness, Anxiety
vs
Apathy, Relaxation, Deliberateness
"hypomania"

Cluster E2
Sthenic emotionality, Hypomania, Instability
vs
Self-control, Patience, Phlegm
"hypomania"

- Cluster E3
  - Intrusiveness, Frivolity, Neuroticism, Instability vs Deliberateness, Seriousness, Reserve — "pre-psychotic hebephrenic schizophrenia"
- Cluster E4
  - General emotionality, Dissatisfied, Intense vs Content, Placid, Temperate — "general emotionality" "physiological and constitutional"

SECTOR F

Gratefulness, Friendliness, Idealism vs Sadism, Slanderousness, Suspiciousness (Benign Cyclothyme vs Hostile Schizothyme)

- Cluster F1
  - Gratefulness, Easygoingness, Geniality vs Hardness, Vindictiveness, Cold-heartedness
- Cluster F2
  - Gratefulness, Kindness, Christian Idealism vs Hostility, Cynicism, Selfish-withdrawal
- Cluster F3
  - Friendliness, Generosity, Cooperativeness vs Hostility, Meanness, Obstructiveness — "obsessional compulsive"
- Cluster F4
  - Cynicism, Suspicion, Dishonesty vs Idealism, Trustfulness, Respecting Self and Others — "paranoid personality"
- Cluster F5
  - Obstructionism, Cynicism, Unstable hostility vs Idealism, Affection, Sensitive consideration
- Cluster F6
  - Benign emotional maturity vs Slanderous, Jealous, Self-pitying, Infantilism — "emotional maturity"
- Cluster F7
  - Sadistic, Vindictiveness, Suspicion vs Temper, Unresentfulness, Complaisance (Paranoid schizoid vs Trusting cyclothyme) — "paranoid personality"

SECTOR G

Liveliness, Insatiability, Verbal Expressiveness vs Reserve, Quiescence, Naturalness

- Cluster G1
  - Austerity, Thoughtfulness, Stability vs Playfulness, Changeability, Foolishness

CLUSTER G2
Verbal Skill, Interesting ideas, Inquisitive
vs
Narrow interests, Absence of Flattery
CLUSTER G3
Eloquence, Affectedness, Conversationalism
vs
Self-effacement, Inarticulateness, Naturalness
CLUSTER G4
Creativity, Wit, Emotional color
vs
Dullness, Banality, Stability
SECTOR H
Imaginative intuition, Curiosity, Carelessness
vs
Thrift, Inflexible Habits, Smugness
CLUSTER H1
Thrift, Tidiness, Obstinacy "anal eroticism"
vs
Lability, Curiosity, Intuition
CLUSTER H2
Creativity, Curiosity, Intuition
vs
Stability, Insensitiveness
SECTOR I
Bohemian, Disorderly
vs
Persevering, Pedantic
CLUSTER I1
Profligacy, Planlessness, Friendliness
vs
Austerity, Hostility, Perseverance
SECTOR J
Aesthetic interests, Thoughtfulness, Constructiveness
vs
____
CLUSTER J1
General aesthetic interests, Thoughtfulness, Constructiveness
vs
____
SECTOR K
CLUSTER K1
Physical strength, Endurance, Courage
vs
Physical inactivity, Avoidance of danger
SECTOR L
CLUSTER L1
Amorousness, Playfulness
vs
Propriety
SECTOR M
CLUSTER M1
Alcoholism, Rebelliousness, Carelessness
vs
Piety, Reverence, Thrift
SECTOR N
CLUSTER N1

Curiosity, Wide interests
vs
Limited Interests

SECTOR O

CLUSTER O1

Hypochondriacal, Taciturn, Retroversion
vs
Eloquence, Interest in future

SECTOR P

CLUSTER P1

Asceticism, Eccentricity — "hermit-eccentric schizophrenia"
vs
Comfort-loving conventionality

SECTOR Q

CLUSTER Q1

Inflexibility, Wandering
vs
Adaptableness, Ease of settling down

In addition to quoting the relationships which Cattell has stated, you will note the necessity of my having added certain interpretative statements to enable our further discussion of certain clusters and traits.

In approaching the work here so briefly described, Cattell has said, "A few such aspects of personality as extraversion, character integration, general emotionality, and various psychotic and neurotic syndromes exist in clinical observation, but they float freely in relation to each other and in an uncharted universe of large possibilities. For alone they contribute very little to the total definition of personality. The problem of primary importance at the outset of systematic factor analysis of personality, therefore, is to find means of choosing so complete a universe of traits that (1) no possible trait will escape detection, and (2) the inter-relations of all important constellations will be given by the analysis."

Feeling that Cattell has, without question, gone far toward the consummation of the task he set for himself, we recognize one cannot possibly study the in-relations of the entire sphere of personality at one time. It is our desire to study together one sector and its clusters. We have chosen the area of Sociability and the commonly related syndrome known as "extraversion-introversion."

We have chosen this area, first, because of its great importance in the organization of human personality, and secondly, because we sense the tremendous confusion which arises from its description in our every-day clinical and consulting activity. Can the study of Cattell's findings in this area help us in our own clinical and consulting descrip-

tion of personality? Certain commonplace observations about the area will enable us to enter into Cattell's findings more readily.

First, the "man in the street" makes clear to us certain facts about the description of Sociability. Ask him to describe for you the personality of a friend, and the chances are tremendous that his first statements will attempt to describe the social patterns of the individual's life, which may be stated very simply as "how he gets along with other people." Any lengthy description on his part or through persistent urging or questioning on yours will result in the "man in the street" telling you "how he gets along with other people" in terms of affective or emotional patterns, or "how he feels"; intellectual or mental patterns, or "how he thinks;" and motor or physical patterns, or "how he does things." And in all probability in this order or sequence.

We are facing here the fact that the area of sociability is a specific area of personality in which the descriptive language used reveals how an individual uses the other areas or factors of human behavior to respond to the specific stimulus of other persons, to that stimulus which man has long recognized as the most important in his environment and which has, therefore, been elevated to a "sector" or specific area of personality itself.

Second, the physiologist and others interested in the biophysical organism, in, and through which the human personality is given expression, have made clear to us that all structures and functions of the biophysical organism are required to explain the reactions and patterns of human behavior usually subsumed under the affective, the mental, and the motor areas of human personality. Some have given emphasis to the developmental unfolding through their discussions of the biophysical organism related to each area, and the possibility of dominating emphasis of certain areas in the total pattern of personality.

We are here facing the fact that there is no specific part of the biophysical organism set aside for the specific function of Sociability as such, but that, again, Sociability is the pattern of response established by each of these three areas in reaction to a specific stimulus of another or other individuals.

Third, the psychological clinician, in attempting the development of psychometric techniques to investigate Sociability in terms of Social Maturation has given emphasis to the same fact. Doll, in the "Vine-

land Social Maturity Scale,"[3] uses a large series of questions, each taken directly from the pattern of affective, mental or motor behavior, but seeks to find in what manner this specific behavior affects the pattern of the individual's response in the social situations or environment. It is not only important to know whether a child eats with his fork and knife; the greater problem of Sociability is whether others in the social group, the family, must disrupt their individual or social reactions to feed him, or whether he uses the eating tools himself and in accordance with patterns acceptable to the social group, and thus causes no disturbance to the family group activity. He may learn to read and to write, but the greater question in Sociability is whether he uses this accomplishment to attain the necessary interchange in communication of ideas to take his place in the social group without failure or disturbance.

The remaining emphases in the study of Sociability have had to do with the learning of patterns of reaction, their organization, their function, and the dynamic aspects of their inter-play in social living.

With these facts in mind, let us turn to the analysis of sector D—Sociability, and its clusters and traits as revealed by Cattell's findings.

It will be observed that the Sociability Sector makes use of 24 trait-pairs, or 14% of the total list of 171 trait-pairs. This might seem to contradict the significant fact that the Sociability Sector could employ all of the traits of human behavior in a specific way. On the other hand, it might reveal the fact that, in our desire to describe this area, we have narrowed it quite significantly. What is this descriptive pattern recorded in Cattell's findings and in the related syndromes of the clinic and laboratory?

First, we must observe that there are five, more or less, specific clusters subsumed under this sector, all of which are commonly referred to under the syndrome "extroversion-introversion." Our first necessity is to clarify the differences which may be found in the five clusters. To analyze the trait-pairs found under each cluster it seems necessary to designate them for the moment as follows:

SECTOR D—SOCIABILITY
CLUSTER D1—General Sociability
CLUSTER D2—General Social Interests
CLUSTER D3—Social Group Interests
CLUSTER D4—Socio-Personal Reaction
CLUSTER D5—Socio-Evaluation Reaction

[3] Doll, Edgar A. Vineland social maturity scale. Vineland Training School. Vineland, N. J. 1935.

Some considerable difference accrues from the description of the Sociability of an individual in terms of D1—a general over-all sociability, or D2—a general interest in social actions, or D3—an interest primarily in specific social group actions, or D4—an emphasis upon the retroflex action of social life upon the temperament of the individual himself, or D5—an emphasis upon the retroflex action of social life upon the individual in terms of an evaluation made by the other members of the social group and in terms of their acceptance or rejection of certain standards of conduct.

This would likewise indicate the same considerable difference in the various patterns of the related parallel or synonymous syndrome of "extroversion-introversion," and in somewhat similar terms. The pattern of "extroversion-introversion" might be described as D1—a general over-all sociability, or D2—a general interest in social actions, or D3—an interest primarily in specific social group actions, or D4—an emphasis upon the retroflex actions of social life upon the temperament of the individual himself, or D5—an emphasis upon the retroflex action of the social life upon the individual in terms of an evaluation made by the other members of the social group and in terms of their acceptance or rejection of certain standards of conduct.

The analytical chart of D Sector's five clusters will enable us to see more readily what trait-pairs are specific or common to each cluster, and what such finding may mean to us.

Cluster D1 is composed of trait-pairs 95, 130, 149, 162, and 150, giving emphasis to the General Sociability of the individual. No single trait-pair is specific to this cluster. It has trait-pairs common to clusters D2, D3 and D4, particularly those involving 150—Seclusiveness-Gregariousness, and 95—General Social Interest. Other traits are shared with clusters under entirely different sectors of personality which will be discussed later.

Cluster D2 has three trait-pairs, numbers 14, 90, and 143, specific to itself and giving emphasis to interest in home and family group activity in an affective or sentimental way. D2 shares its emphasis upon 95—general interest in social life—along with D1, 132—Responsiveness-Aloofness—with D3; and an emphasis on 83—Dependence-Independence—with D3. One of these and other traits are shared with other clusters under entirely different sectors of personality.

Cluster D3 has no trait-pairs specific to itself. It shares the trait

CHART 1

ANALYTICAL CHART OF SECTOR D—SOCIABILITY (CATTELL)

Trait List Number	Trait Pairs	D Clusters	Related Clusters
14	Affectionate-Frigid	D2	
90	Interest in Home and Family	D2	
143	Sentimental-Hard-hearted	D2	
114	Optimistic-Pessimistic	D5	
95	Interest in Social	D1D2	
132	Responsive-Aloof	D2D3	
104	Laughterful-Mirthless	D4D5	
66	Frank-Secretive	D2	AC2
69	Genial-Cold-hearted	D2	F1
23	Ascetic-Sensuous	D4	P1
92	Interest in Physical Activity	D4	K1
169	Witty-Humorous	D5	G4
130	Reserved-Intrusive	D1	E3,G1
149	Shy-Sociable I (Forward)	D1	CB3,CB4
162	Bold-Timid	D1	CB1,CB3
142	Sensitive-Tough	D2	AC1,CB4
40	Cooperative-Obstructive	D3	F3,F5
109	Mischievous-	D4	G1,L1
144	Serious-Frivolous	D4	E3,G1
57	Enthusiastic-Apathetic	D5	CB1,E4
67	Friendly-Hostile	D2	CA3,F2,F3,I1
83	Dependent-Independent	D2D3	B1,B3,CB1,CB2,CB5
31	Cheerful-Gloomy	D4D5	AB3,AC1
150	Seclusive-Sociable II (Gregarious)	D1 D3D4	AC1

132—Responsiveness-Aloofness—with D2; 83—Dependence-Independence—with D2; and 150—Seclusiveness-Gregariousness—with D1 and D5. Some of these and other traits are shared with a number of clusters under entirely different sectors of personality.

Cluster D4 has no trait-pairs specific to itself. It shares trait-pair 104—Laughterful-Mirthless—with D5; and 31—Cheerful-Gloomy—with D4. Some of these and other trait-pairs are shared with clusters under entirely different sectors of personality.

Cluster D5 has specific to itself trait-pair 114—Optimistic-Pessimistic. It shares 104—Laughterful-Mirthless—with D4. All other trait-pairs are shared with other D clusters, as 31—Cheerful-Gloomy—with D4, and, at the same time, with a number of clusters under entirely different sectors of personality.

By now, we must be convinced of the scarcity of traits definitive of specific aspects of sociability or "extroversion-introversion." I am

fully appreciative of the fact that I am not clarifying the descriptions by this detailed analysis. For the time, I am asking you merely to observe the relationships involved.

When we turn to the trait-pairs shared by the five Sociability clusters with other clusters under entirely different sectors of personality, we shall observe the added confusion in syndrome description of personality.

Cluster D1—General Sociability—shares its very basic trait-pair 130—Reserved-Intrusive—with E3—"prepsychotic hebephrenic schizophrenia" and G1—stability of a thoughtful type; its most basic trait-pair 149—Shy-Forward—with CB3—"general inhibition" and CB4—"hyper-thyroid hypersensitivity"; 162—Bold-Timid—with CB1 "somatotonic" and CB3 "general inhibition"; and 150—Seclusive-Gregarious—with AC1 "constitutional agitated melancholia."

Cluster D2—General Social Interests—shares 66—Frank-Secretive—with AC2 "prepsychotic catatonic schizophrenia"; 69—Genial-Cold-hearted—with F1—a pattern of Gratefulness and Geniality; 142—Sensitive-Tough—with AC1 "constitutional agitated melancholia" and CB4 "hyper-thyroid hypersensitivity"; 67—Hostile-Friendly—with CA3 "assertiveness-submissiveness," F2—idealistic gratefulness, F3 "obsessional compulsiveness" and I1—a pattern of Bohemian Disorderliness; and 83—Independence-Dependence—with B1—the emotionally toned pattern of "emotional maturity," B3—the volitionally toned pattern of "emotional maturity," CB1 "somatotonic," CB2 "somatotonic," and CB5—a pattern of sophisticated behavior.

Cluster D3—Social Group Interests—shares 40—Cooperative-obstructive—with F3 "obsessional compulsive," and F5—a pattern of affectionate idealistic behavior; 83—Dependence-Independence—with B1—emotionally toned "emotional maturity," B3—volitionally toned "emotional maturity," CB1 "somatonic," CB2 "somatotonic," and CB5—a pattern of sophisticated behavior; and 150—Seclusiveness-Gregariousness—with AC1 "constitutional agitated melancholia."

Cluster D4—Socio-Personal Reactions—shares 23—Ascetic-Sensuous (comfort loving) with P1—a pattern of ascetic behavior; 92—Interest in Physical Activity—with K1—a pattern of physical activity and endurance; 109—Mischievousness—with G1—stability of a thoughtful type, and L1—a pattern of propriety; 144—Seriousness-Frivolousness—with G1—stability of a thoughtful type, and E3 "pre-

psychotic hebephrenic schizophrenia"; 31—Cheerful-Gloomy—with AB3 "general neuroticism," and AC1 "agitated constitutional melancholia"; and 150—Seclusiveness-Gregariousness—with AC1 "agitated constitutional melancholia."

Cluster D5—Socio-Evaluation Reactions—shares 169—Witty-Humorous—with G4—a pattern of creative stability; 57—Enthusiastic-Apathetic—with CB1 "somatotonic," and E4 "general emotionality" of a constitutional and physiological type; and 31—Cheerful-Gloomy —with AB3 "general neuroticism and AC1 "constitutional agitated melancholia."

The overlap of syndromes in use of certain trait-pairs may be observed as follows:

Cluster	*Syndrome* related by *number of trait-pairs*	
AB3	"general neuroticism"	1
AC1	"constitutional agitated melancholia"	3
AC2	"pre-psychotic catatonic schizophrenia"	1
B1	"emotional maturity" (emotionally toned)	1
B3	"emotional maturity" (volitionally toned)	1
CA3	"assertive-submissive"	1
CB1	"somatotonic" (1)	3
CB2	"somatotonic" (2)	1
CB3	"general inhibition"	2
CB4	"hyper-thyroid hypersensitivity"	2
CB5	(pattern of sophisticated behavior)	1
E3	"prepsychotic hebephrenic schizophrenia"	2
E4	"general emotionality"	1
F1	(pattern of grateful geniality)	1
F2	(pattern of grateful idealism)	1
F3	"obsessional compulsive"	2
F5	(pattern of affectionate idealism)	1
G1	(pattern of thoughtful stability)	3
G4	(pattern of creative stability)	1
I1	(pattern of bohemian disorderliness)	1
K1	(pattern of physical endurance)	1
L1	(pattern of propriety)	1
P1	(pattern of asceticism)	1

Of the 24 trait-pairs found under the five D-Sociability clusters, 7 are specific to D clusters alone or in combinations, while 17 are shared with a total of 23 other clusters or syndrome patterns of known observable personality reactions.

Let us assume that we are observing the behavior of an individual which we might describe as falling under the trait-pairs 150—Seclusive-Gregarious, 31—Gloomy-Cheerful, and 142—Tough-Sensitive. We would note that, in combination with certain other trait-pairs, we might readily refer the behavior to one or two clusters of 4 possible

clusters under D—Sociability, either D1, D2, D3, or D4; or observed with certain other trait-pairs we might refer the behavior to the cluster AC1—"Constitutional agitated melancholia." It must be clear to us that, even under the cluster AC1—"Constitutional agitated melancholia," we are observing one of 4 possible forms of Sociability reaction now recognized and reported. It is quite possible that there are other specific Sociability clusters not yet recognized to which this series of trait-pairs might also readily refer.

Such cross-relationships would be observed among the 23 clusters and the 5 D-clusters, if we should take time to carry out all possible combinations of personality reactions.

Our present analysis, I trust, must somehow force upon us the conviction that the clinician, or consultant, is seldom so thorough in the basic description of behavior reactions. At the same time, it must convict us of our own carelessness when confusion arises in the syndrome description of personality patterns.

Cattell has pointed to the possibility of more exact studies based upon experimental research under this method of more precise description of personality. The clinician and consultant should likewise be able to profit greatly in his analytical diagnosis under this form of more precise description of personality. It would repay the clinician and consultant to spend the time necessary to thoroughly familiarize himself with the findings now available through the analysis of Cattell's studies and the related phenomenological studies. The clinician and consultant must also face the challenge to make his much-needed contributions in the more accurate clinical description of personality, which are long awaited and necessary to the final solution of this gigantic task.

SECTION OF ANTHROPOLOGY

October 22, 1945

Doctor Erwin H. Ackerknecht, American Museum of Natural History, New York, N. Y.: *Primitive Medicine.*

One of the most important aspects of primitive medicine is the *pharmacopoeia* of most primitive tribes. Except for a very few regions, like the Arctic and Melanesia, where drug lore has been developed only slightly, an amazing percentage of the herbs, barks and roots used by the natives—a percentage which is far above the mathematical probability of random sampling—is of objective medicinal value.

Even today, our own pharmacopocia is heavily indebted to the primitives. Picrotoxine, the powerful stimulant of the respiratory center; strophantine, the well known medicament in heart diseases; emetine, the alkaloid of ipecacuanha and specific in amebic dysentery—all are of Indian origin. Salicylic preparations for rheumatism were first used by the Hottentots, etc., etc. Yet our culture, which, somewhat prematurely, but all the more firmly, believes that the test tube is superior to the plant cells in synthesizing drugs, refuses, in general, to analyze the primitive material which anthropologists and missionaries have brought back from the field. A large scale analysis of primitive drugs would be of great practical importance to modern medicine.

All human societies, primitive and civilized, suffer from disease. Disease is much older than man. It is one of the fundamental, vital problems which face every society, and every known human society develops methods to deal with disease, and thus creates a medicine. But the attitude towards disease and the methods of fighting disease vary enormously in the different primitive tribes. Disease may be of extreme concern to a society, quite beyond its objective frequency. Anxieties arising from other sources may be projected into medicine. The Navahos are said to spend one-fourth to one-third of their productive time in religious ceremonials, most of which are concerned with disease. Disease and its healing or prevention plays a similar preponderant role in the religions of the Cuna, Chiricahua-Apache, Cherokee, Pima, and Liberian Manos. It is the main concern of the Pit River Indians of Northeastern California and, next after sex, of the

Yavapei. The attitude towards disease may take a strange twist, as in Dobu, where healing practices are subordinated to the all-absorbing problem of how, by appropriate spells, one can make one's neighbor sick! The attitude toward disease may change under our very eyes, as Redfield describes it for Yucatan, when acculturation occurs. Disease theories, diagnostic and therapeutic measures, all may vary widely. There may, or may not, be special medicine men. They may be poor or rich, all-powerful or uninfluential. Here, the magic power of the drug may be the central element of native medicine. There, the power of the healer or of the medical society.

We are accustomed to look at disease as a purely biological phenomenon, and at medicine as a kind of reflex reaction toward it. These differences in approach are therefore extremely disturbing to us, and all the more so as they are not founded primarily on the objective amount of disease in a given tribe. We may encounter various attitudes combined with about the same kind and amount of morbidity, and in very similar climatic and economic conditions. Under such circumstances, medicine has much more clearly the character of a function of the culture pattern than of the environmental conditions.

Though it is necessary to bear in mind these underlying variations, it is nevertheless possible to make a few generalizations concerning primitive medicine, especially with reference to some of their functional and psychological implications. I should like, however, to remind you of the following facts:

Disease and death among primitives are, for the most part, not explained by natural causes, but by the action of supernatural forces. In general, the disease mechanisms are either the intrusion of a disease-producing foreign body or spirit, or the loss of one of the souls which may be abducted or devoured. These mechanisms may be put into motion either by a supernatural agency (God, spirit, etc.) who feels offended, or by a fellow man who avenges himself either by hiring a sorcerer or by himself acting as a sorcerer. Supernatural causes must be discovered by supernatural means, and thus the primitive diagnosis consists of various types of divination—bone-throwing, crystal-gazing, trances, etc. The therapeutics cover a whole gamut of methods, reaching from purely matter-of-fact treatments (herbs, massage, bath, etc.), combined with magic spells or prayers, to purely magico-religious rites.

One of the common traits of primitive medicine, which makes it

rather different from ours, is the *social role* that disease and medicine assume in primitive society. Disease with us is, in the last analysis, a biological, individual, and non-moral problem. No guilt is involved when we suffer from hereditary, infectious, or degenerative diseases. Even in venereal diseases, we strive to eliminate the moral aspect as it has proved to be a handicap in their eradication. If you get appendicitis or cancer, you will never think of associating this with your behavior toward your neighbor or mother-in-law or your ancestral spirits. We do not usually associate disease with whether or not our personal relations are good, whether we keep certain religious or social rules or not. But this is exactly what the primitive does. Disease derived from sorcery, from taboo violation, from the anger of ancestral or other spirits is the expression of social tensions. A seemingly independent, biological problem is thus woven into the whole socio-religious fabric in such a way that disease and its healer play a tremendous social role, a role that, in our society, is assumed rather by judges, priests, soldiers, and policemen.

In many primitive societies, disease becomes the most important social sanction. Primitive medicine contains a moral element which is almost absent in ours. "Be peaceful, pay your debts, abstain from adultery, in order to protect yourself *and your family* from disease." It thus becomes possible to treat disease by pacifying offended persons. New light is here thrown on the marked interest of the primitive community in the diseased person and its participation in healing rites.

This social role of disease may also partly explain the persistency of primitive medicine, quite apart from its intrinsic medical value. The purely curative effect of certain rites may be negligible, but they are upheld because they fulfill important social functions.

In a way, disease, being thus regarded as a direct consequence of personal, social behaviour, makes more sense to primitive man than it can make to the patient in our society. Only rarely, are we able to relate the non-personal biological notions, which to us explain disease, directly to the actual life history of the patient. There is a tendency of the patient and his family in our society, well known to every medical practitioner, to bridge this gap, in constructing such relations even where they actually do not exist. You all might remember cases of cancer being referred to a slight and perfectly irrelevant bodily traumatism, cases of epilepsy explained by fright, etc. This tendency has

disappeared from medical science only gradually and at a relatively late period.

The social concept of disease in primitive society is also reflected in the belief that the disease sanction may affect every member of the family as well as the sinner himself. This is a far more inclusive notion than our concept of hereditary or infectious diseases. Therapeutic measures—whether it be confession or medication—have, therefore, very often to be applied not only to the patient, but to his whole family.

It is obvious how this specific social role, assumed by disease in primitive society, contributes to the formation of a type of medicine which differs considerably from the one to which we are accustomed.

I think it is safe to state, as a further general characteristic of primitive medicine, that it is *primarily magico-religious, utilizing a few rational elements;* while our medicine is predominantly rational and scientific, employing a few magic elements.* We are naturally inclined to think of primitive medicine in terms of rationality, just as primitives usually interpret our medicine in terms of magic. It is not difficult to see that both procedures are projections. One can, of course, argue that our medicine is magic, or that theirs is rational; but both statements need closer examination and qualification. I believe that I am more aware of magical elements in our medicine than many of my medical colleagues are. For the anthropologists, it is not difficult to discover the magic character that vitamins, germs, number complexes, etc. often assume in the mind of the *patient.* But one of the important strictures which have to be made is that *magic elements in our medicine are overwhelmingly on the side of the patient.* As far as our medical system and its representatives are concerned, they remain rational in intent, content and procedure. In order to obtain a clear picture, it is also necessary to look at the problem from a more quantitative angle, and I doubt whether anyone would seriously argue that the modern patient's approach to medicine and disease is *predominantly* magico-mystical.

Nobody will deny that, in a great number of primitive tribes, not all diseases are interpreted in a supernatural manner and that some are regarded as due to natural causes. This holds true, especially, for very common diseases, such as colds, toothache, malaria, etc., those resulting from old age, and those of which the imported character is

* I am using the term "rational" not as a mere equivalent of logical—magic is logical in its way too—but, as it is now commonly understood, logical on the basis of empirical premises.

clearly realized. Yet the inconsistency with which such ideas are used is noteworthy. The same disease might be naturally or supernaturally caused, and a natural disease might be treated supernaturally, or vice versa. It is also remarkable that positive knowledge concerning the "natural diseases" is about as poor as that concerning "supernatural diseases" and that primitive sceptics, in general, doubt the supernatural character of individuals or isolated events, but not the whole supernatural system.

There are a certain number of purely rational treatments, such as bleeding, massage, and drugs. Yet the number of such treatments decreases considerably after closer examination and appears at least to be "mixed." The rational character of the use of the tourniquet in snake bite among the Liberian Manos becomes, for instance, somewhat doubtful, when we hear that a ring of white clay might also be applied around the bitten limb. Rivers, observing an apparently rational abdominal massage for constipation, on Eddystone Island, was very disappointed in learning that it was destined to drive out a magic octopus. Another case in point is the Cherokee patient, who keeps a strict diet all day long, but devours everything at night when the taboo underlying his diet is no longer valid. The widespread division between medicine men and herbalists in primitive tribes has given rise to the premature conclusion that only the former are guided by supernatural ideas, while the latter are rationalists. This thesis seems to be unsupported by the facts.

The *fundamental error* in all this reasoning about primitive rationalism is the basic assumption that *what is objectively effective, is also rational and scientific*. Also, there are so many objectively effective elements in primitive magic treatments, such as, for example, drugs, baths, massage, sucking, bloodletting, isolating of infectious diseases, diet, inoculation against snake bite, etc., such treatments are without hesitation christened rational or even scientific, whereas they might be magical or purely habitual, almost automatic or reflex-like. Yet this identification of the objectively effective, the rational and the scientific can be nothing but a permanent source of confusion. The attribute of rational should be reserved for actions which are actually based on thought identifiable with our rational thinking. The notion of scientific should be handled with the same sense of discrimination. Even animals use objectively effective healing methods, while science has

nothing of an instinctive reaction. Linton has lately very justly written of science as an *invention.* One might as well speak of scientific thought as a great and late revolution in human behavior. Science aims primarily at truth, not at success or psychic relaxation. Science is unthinkable without a certain amount of individualism. It means a complete misunderstanding of scientific thought and methods, and of the whole history of science, to bestow the name of scientific, or even rational, upon practices which are, in general, uninfluenced by experience, free from scepticism, and where no numerical notions, no abstraction, no induction, and no systematization are underlying ideas. Calling every objectively effective procedure scientific seems to me to dilute the notion of science almost to complete meaninglessness.

This misunderstanding is also favored by the assumption that supernatural, irrational ideas and practices are always highly emotional. Thus, unemotional behavior ought to be rational. But experience with primitives shows that magic might be something highly unemotional.

I fully agree, therefore, with the following statement of the great American medical historian, Fielding H. Garrison:

> "If we are to understand the attitude of the primitive mind toward the diagnosis and treatment of disease, we must recognize that medicine, in our sense, was only one phase of a set of magic or mystic processes, designed to promote human well being, such as averting the wrath of angered gods or evil spirits, fire making, making rain, purifying streams or habitations, fertilizing soil, improving sexual potency or fecundity, preventing or removing blight of crops and epidemic diseases."

Isolated rational elements, the existence of which nobody denies, do not and are not able to change or even to influence considerably the fundamental character of this magico-religious system.

It is for pragmatic reasons that I have given so much importance to what might seem a quite superfluous discussion of terminology. Primitive medicine's contribution to our medical understanding consists, in addition to its great treasury of drugs, just in the fact that here we are able to observe a whole system of medicine, different from ours and yet functioning with considerable success. It is, perhaps, because of this difference, which seems to me greater than in other fields like religion, art, or law, that the study of primitive medicine has been rather neglected.

It is true that in merely calling primitive medicine, "magic," not much is accomplished. Limitations of space and time make it im-

possible for me to enter here a detailed discussion of magic. But I cannot abstain from alluding to the fact that, in my opinion, under the heading of magic, at least two very different sets of practices and beliefs have been lumped together.

I would like to mention here a few of the many effects which supernaturalism has on medical practice. The extreme *ignorance of anatomical facts* among primitives has often been commented upon, and has been attributed to the absence of dissections. Quite apart from the fact that experience in hunting, sacrifices, and cannibalism could well compensate for this shortcoming, I have been able to point to four areas in Siberia, Oceania, South America and West Africa where autopsies are practised quite extensively in order to discover whether the deceased died as the result of witchcraft or not. Yet these dissectors are just as ignorant of the most elementary anatomical facts as are their non-dissecting brethren. It is not a mere technique, but a complete change in outlook that almost brings about a change in perception and opens the eyes of the super-naturalist to anatomical facts.

There is, in general, satisfactory wound and fracture treatment among primitives, but almost nothing which resembles our *major surgery* (amputations, excisions of tumors, abdominal surgery). In the few places where such major surgery is practiced, it is mostly of very low quality. Here again, technical explanations are insufficient. Primitives certainly do not lack technical skill, as is shown by the widespread and successful performance of trepanation, an operation which had an extremely bad prognosis, even with western surgeons, until a few decades ago. Neither do primitives lack occasions to gain surgical experience, because of numerous accidents, ritual and judiciary mutilations occurring in primitive society. The greater resistance of those individuals who reach adult age in primitive societies, and the absence of particularly virulent bacterial strains as we cultivated them throughout centuries in our hospitals, would be a special asset for primitive surgeons. But the magic fear to appear mutilated in the spirit world is an all-powerful obstacle to the development of surgery. This fear creates opposition even to tooth extractions. For example, Linton says of the Tanala that they do not fear death, but mutilation. It is this supernatural character of mutilation which makes it so particularly an impressive form of punishment; and, in turn, people refuse amputation which would put them on the level of criminals.

Supernaturalism also makes for a strange *inflation of the pharmacopoeia,* where the effective and the ineffective are used without discrimination. This obvious lack of experimentation reduces claims of primitive rationalism that otherwise could find strong support in the primitive's use of effective drugs.

While supernaturalism, in general, serves as an obstacle to technical improvements in medicine, it sometimes brings them about as unintended by-products, as is shown by circumcision, certain forms of sex hygiene, and other sanitary measures. The most striking example of this kind is probably the invention of the *incubator for premature births* by certain Eskimos and Bantus. This invention seems to have arisen from particularly stringent taboos against miscarriage prevailing in these two tribes, otherwise so different from each other.

The irrational need not be ineffective, and with all its peculiarities, primitive medicine, which, by the way, in terms of space and time, covers a much larger field than does our scientific medicine, seems to have *served its purpose more or less satisfactorily.* In some places and periods, it seems to have been even superior to our medicine. Some of its successes (as are those of our medicine) are undoubtedly due to the fact that man is a fairly solid animal, and many diseases are self-limited anyway. The aforementioned objectively active elements in primitive medicine and its particular social mechanisms may account for other successes.

One particular character of primitive medicine, which is derived from primitive mentality, in general, and which makes description of primitive medicine on the basis of our categories so difficult, deserves special notice in this context. This is the *unitarian or total character of primitive medicine.* When, for practical reasons, for instance, we have spoken above of diagnosis and therapeutics, of bodily and mental diseases, we have done violence to the facts. Actually, there is no diagnosis separated from therapeutics in such acts as divination or confession. The diagnostic act *is* at the same time a therapeutic one; and that old dichotomy between mental and bodily disease, which we seem largely unable to overcome even with psychosomatic medicine, just does not exist among primitives, either in pathology or in therapy. The whole individual is sick and the whole individual is treated. This particular form of integration offers undoubtedly certain therapeutic advantages, as disease is fundamentally a process of disintegration on

all levels, the physical, mental, and social. Magic or religion seems to satisfy better than any other device a certain eternal psychic or "metaphysical" need of mankind, sick and healthy, for integration and harmony. The non-empirical character of primitive medicine also provides it with an element of certainty which undoubtedly, gives it considerable curative powers.

We are inclined to isolate certain elements of primitive therapeutics, to which I have just alluded, under the label of *psychotherapeutics.* We easily discover in most primitive treatments certain effective mechanisms, which we have applied consciously and on a large scale for a number of years in our own medicine, such as *suggestion and confession.* We have gained a certain understanding of these processes. Through work initiated by men like Pavlov, Cannon, or Freud, we have learned a good deal about the importance of certain mental stimuli for coordination of the whole organism and of the catastrophic physiological consequences of conscious and unconscious fear. In addition, two wars have provided many of us with a certain amount of altogether undesired practical experiences in this respect. It is in the nature of things or, more exactly, it is a consequence of the particular ways in which our sciences and our society have developed that, in no other branch of medicine, do primitive and modern medicine overlap to such an extent as in psychotherapy. We started much later in dealing scientifically with psychological and social problems than with those of matter. The decline of official religion has brought into the office of the doctor many problems which were formerly handled by the priest. A number of years ago Opler published a very interesting analysis of the methods of an Apache medicine man in the light of modern psychotherapy, pointing to parallels as well as to differences. Without denying to modern psychotherapy any of its successes and its merits, I am rather inclined to think that it labors under many delusions as to the degree of rationality it has reached. Many of its cures seem to me due far less to its rationalizations than to the simple mechanisms of suggestion and confession, also underlying certain practices of the medicineman.

We have insisted, above, on the social determinants of primitive medicine; but every consideration of primitive medicine that does not at least try to gain an insight into the biological bases of this medicine, that is, the *pathology* of primitives, would be utterly incomplete.

Everybody who has ever tried to study so-called racial pathology knows that, in no field of primitive medicine, are the data as scanty and contradictory as in this one. Nevertheless, it seems safe to state that truly primitive communities, perhaps because of their relative isolation, of the lower life expectancy of their members, show a relatively low morbidity of their adult population. This has been the impression of qualified observers, not only in the Arctic, Polynesia or North America, but even in such unhealthy regions as South America or Africa.

We must not forget that the same process, which ultimately brought about the development and diffusion of a highly qualified medical science, meant primarily a tremendous increase and spread of disease all over the world. This continent probably knew nothing of measles, smallpox, malaria, yellow fever, cholera, pest, and many other infectious diseases, before its discovery by the white man. Infections were the most deadly automatic weapon of the European in his conquest of the country. It is of historical interest that one of these infections, small pox, on a few occasions, in both North and South America, was used consciously in the warfare against the Indian. It is true that primitive medicine proved, in general, unable to adapt itself to these new situations. Application of traditional methods, like baths in the case of small pox, even had particularly fatal consequences.

Degenerative diseases, which form such an important part of our pathology, had little chance to develop in populations which had an average life expectancy well below 30. Besides frequent accidents and other forms of violent death, this low life expectancy is undoubtedly due to a high infant mortality, which is known and feared, but badly controlled by primitives.

Mental disease, at least in the form of our psychoses, particularly schizophrenia, progressive paresis, or delirium tremens, seem equally rare among primitives. On the other hand, the diseases called "functional" in our terminology often seem particularly frequent. They, together with rheumatism, digestive disorders, colds and respiratory diseases, skin and eye affections, and gynecological disturbances, seem to form the stock-in-trade of primitive pathology. This, and not the post-conquest situation, is the background against which the accomplishments of primitive medicine should be measured. Such a comparison leaves a far more favorable impression of the adequacy of primitive medicine.

I cannot leave my subject without saying at least a few words about the primitive medical practitioner, the *medicine man.* For a long time he has been labeled in anthropological literature, even of the highest caliber, e.g., Tylor, as a fake. I think this point of view has now completely died out and, therefore, need not be insisted upon. We know now that the percentage of fakes among medical practitioners is not higher in primitive than in other societies. We have won a juster appreciation of the so-called tricks of primitive medicine men, that were so highly offensive to early travelers and missionaries, by understanding their symbolical character. It is also interesting to note that less articulate parts of the population have never shared in this contempt for the medicine man. The "Indian Doctor," that is, the native medicine man or an Indian-trained white lay practitioner, was a very widespread phenomenon in frontier settlements throughout the first half of the 19th century, and when Indians were removed to reservations, white settlers petitioned for exception of their Indian Doctor. At early AMA conventions of the 1840's, reports on Indian pharmacopoeia were read. The intensive government-sponsored study of Indian medicine by Hernandez in Mexico and by Piso in Dutch South America, during the 16th Century, belong in the same category.

Another, hardly less complimentary or correct label for the medicine man, that of "psychopath" or "epileptic," still lives in many anthropological textbooks or articles.

I am unable to take up, here, the whole complicated problem of abnormality in primitive society. I refer you to the excellent work of Ruth Benedict, Hallowell, and later writers, which forms one of the most valuable contributions modern anthropology has made to the problems of medicine, psychopathology, and psychology in general. I would, nevertheless, like to stress one technical question which, so far, has been given little attention and which might have played an important role in the genesis of this misunderstanding. Mental disease in primitives, recognized as such by both natives and whites, manifests itself often in so-called states of involuntary possession; that is, paroxysms, accompanied by spastic motions, incoherent language, etc. To the non-initiated, these accidents look like epilepsy, though actually they are not, and fall rather in the group of diseases that we usually call hysteria. Many medicine men, too, are subject to trances and states of possession. Though these forms of possession are voluntary, very

often induced by drugs, perfectly normal and clearly differentiated from the possession-disease by many tribes, to the untrained observer they look very similar to epilepsy or hysteria. I suspect that, through this confusion, the medicine man has undeservedly acquired the title of an epileptic or psychopath.

There is no doubt that psychopathology plays an important role in the life of certain groups of medicine men, the best known of whom is the Siberian shaman. In view of the very special character of the Siberian shaman, it is extremely regrettable that the term, shaman, has been used very loosely as a synonym for medicine men in spite of the protests of Loeb and others. Closer study of the Siberian shaman reveals the perplexing fact that the individual who is to become a shaman passes through a stage of very marked mental disturbance, but that he is more or less cured of this affliction by becoming a shaman.

Even without detailed discussion of the problems of abnormality, it should be fairly obvious that an individual who successfully plays such an important role in society, as the medicine man actually does, cannot, because of mere similarities, be put on the same level as our mentally diseased individuals, whose main characteristic is that they are unable to function successfully in society.

The successes of the medicine man cannot be fully understood, unless one realizes that, acting in small communities, he possesses a more perfect personal knowledge of his patient than most of our doctors do, and that his non-medical activities greatly enhance his authority as a healer.

We hear a great deal about specialists in primitive medicine. It should be realized that this form of specialization seems just the opposite of ours. We have specialists because the volume of our knowledge has become so great that one man can no longer handle all of it. Specialization among primitives seems to result from the fact that there has not yet developed a total body of medical knowledge. One man knows only one or a few spells and practices for one or a few diseases, and thus becomes a specialist for this or that disease.

SECTION OF PHYSICS AND CHEMISTRY

October 12 and 13, 1945

Conference on "*Amino Acid Analysis of Proteins.*"

The Section of Physics and Chemistry held a Conference on "Amino Acid Analysis of Proteins," as the first in the series for the Academic Year 1945–1946. Doctor William H. Stein, Rockefeller Institute for Medical Research, New York, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Introductory Remarks, by William H. Stein.

"The Contribution of the Analytical Chemist to Protein Chemistry," by Hubert Bradford Vickery, The Connecticut Agricultural Experiment Station, New Haven, Connecticut.

"The Use of Specific Precipitants in the Amino Acid Analysis of Proteins," by William H. Stein and Stanford Moore, The Rockefeller Institute for Medical Research, New York, N. Y.

"The Isotope Dilution Method for Amino Acid Analysis," by David Shemin, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Chromatographic and Ion Exchange Methods in Amino Acid Analysis," by R. Keith Cannan, College of Medicine, New York University, New York, N. Y.

"Microbiological Methods in Amino Acid Analysis," by Esmond E. Snell, College of Agriculture, University of Wisconsin, Madison, Wisconsin.

"Amino Acid Composition of Simple Proteins," by Erwin Brand, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Concluding Remarks, by Hans T. Clarke, College of Physicians and Surgeons, Columbia University, New York, N. Y.

NEW MEMBERS

ELECTED FROM MAY 15 TO OCTOBER 25, 1945

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TRANSACTIONS
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SECTION OF GEOLOGY AND MINERALOGY

NOVEMBER 5, 1945

DOCTOR MORLEY E. WILSON, Geological Survey of Canada, Ottawa, Ontario, Canada: *The Regional Structural Relations of the Ore Deposits of the Noranda District, Western Quebec.* (This lecture was illustrated by lantern slides)

Up to a few years ago, most geological maps of Archaean areas in the southern part of the Canadian Pre-Cambrian Shield showed the distribution of formations, but gave little information regarding their structure. In recent years, it has been discovered, however, that Archaean rocks are characterised by features from which their structural succession can be determined. These are mainly pillow and brecciated flow tops in lavas and change of grain and cross-bedding in sediments.

One of the regions in the Shield that has been studied most intensively, during the last 15 years, lies along the fault zone that extends from Kirkland Lake in Ontario eastward for 130 miles to Louvicourt Township in Western Quebec. Most of the zone has been remapped on scales ranging from 500 to 1,600 feet to 1 inch, and it has been ascertained—

(1) That the superficial Archaean rocks of the Noranda-Rouyn

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area in Western Quebec belong areally to three divisions: the Timiskaming Series, the Abitibi (Keewatin) Series, and the Pontiac Group. The Timiskaming Series rests unconformably on both the Abitibi Series and the Pontiac Group, but because of their separate distribution, the relationships of the Abitibi Series to the Pontiac Group is not known. The Pontiac either belongs to a third separate Series, or is a part of the Abitibi Series;

(2) That the zones of carbonate-talc-chlorite alteration with which many of the ore deposits of the Kirkland Lake-Louvicourt fault zone are associated lie along overthrust faults, which developed as an accompaniment of the Pre-Cambrian mountain folding in the region;

(3) That the structure of the Kirkland Lake-Louvicourt fault zone in Western Quebec centers around the Cadillac-Bouzan Lake fault, and, in Ontario, around the Larder Lake "Break." These are in alignment with one another and underlie a deep depression beneath the late Pre-Cambrian Cobalt Series, which forms extensive hills and ridges near the inter-Provincial boundary between Ontario and Quebec. Obviously, they belong to the same fault zone and may be parts of a single fault;

(4) That the fault zones usually contain disseminated pyrite or arsenopyrite and gold in places, but ore deposits of workable extent and grade occur only where a change in strike, a change in wall rock, increased local deformation, or other structural features have permitted more extensive and concentrated deposition from ore-bearing solutions.

We are at the beginning of a new era in Canadian Pre-Cambrian geology, for geological maps, now, not only show the distribution of formations, but their structure as well, and thus direct attention to localities where ore deposits may be present, and development work by diamond drilling or in other ways, may be undertaken. They thus assist more directly than formerly in mineral discovery.

SECTION OF BIOLOGY

NOVEMBER 12, 1945

DOCTOR GEORGE GAYLORD SIMPSON, Chairman, Department of Geology and Paleontology, The American Museum of Natural History; and Professor of Vertebrate Paleontology, Columbia University: *Tempo and Mode in Evolution.* (This lecture was illustrated by lantern slides.)

INTRODUCTION

The title assigned to me for this paper will be taken as symbolic rather than descriptive. "Tempo and Mode in Evolution"[1] is the name of a book by me, completed in 1942, and published last year. This book attempts to take some of the data available to paleontologists and not to geneticists, and to relate these to recent developments in general evolutionary theory. These data have mainly to do with rates of evolution, including such phenomena as inertia and momentum; and with patterns of evolution, including the modes of radical transformation or of differentiation of populations over long periods of time. It is not proposed here to repeat what has already been discussed at length in this published work. What merit the book may have probably arises for the most part from its attempt to synthesize results obtained in two widely distinct fields of investigation—paleontology and genetics. The aims of this paper will be to give some of the historical background of modern evolutionary theories, to follow the divergence of paleontological and genetical thought on this subject, and to say something about the recent movement by which they are again converging on common problems, with great hope for fruitful future cooperation.

THE EVOLUTION OF EVOLUTION

The major thesis of pure Darwinism, the Darwinism of Darwin, was that individuals vary and transmit their variations to their offspring, that some individuals produce more and some less than their proportionate share of young, that those leaving relatively few or no offspring are, on an average, different from those that do produce the

bulk of the next generation, and that the new generation, therefore, includes fewer of some variants and more of others than did the parental group. This differential, continuing over many generations, involves steady structural change in the group as a whole, and this change is evolution. "Natural selection," the usual label of Darwinian explanations of evolution, is a subsidiary theory seeking to explain why some individuals contributed more than others to the heredity of following generations. Darwin did not know how variations arise, how they are inherited, or which variations are heritable and which are not. If it be granted that heritable variations do exist and that new variants do occasionally appear, facts empirically established then, and now confirmed, this ignorance regarding heredity did not at all invalidate Darwin's theory, but it made it quite incomplete and open to question, in application to particular evolutionary sequences.

Darwin's Darwinism was eclectic and it did not exclude the hypothesis, generally labeled as Lamarckian, although it was broadened beyond the scope originally given it by Lamarck, that heritable variations arise by the interaction of an individual and its environment. Variations arising in this way could supply materials for natural selection, but if hereditary variations do arise in this way, it would be possible for a population to evolve without the action of selection. The later 19th century naturalists tended to develop the ideas of selection and of modification by individual interaction with the environment as two antithetical schools, restricting Darwinism to the first, and calling the second Lamarckism. The separation became complete when Weismann promulgated his germ theory (1892).[2] He correctly identified the physical basis of heredity and, on logical rather than experimental or observational grounds, he concluded that somatic modifications could not affect heredity.

Weismann's neo-Darwinism was more Darwinistic than Darwin It excluded any Lamarckian (or neo-Lamarckian) factor and made natural selection not merely the essence of the theory but the whole theory. Darwin, himself, did not envisage natural selection as a complete and sufficient cause of evolution and, even by 1892, many difficulties had been found in accepting so simple an explanation. Weismann's extremism made the situation still less satisfactory. While he distinguished more clearly than had Darwin between hereditary and non-hereditary variations, Weismann was largely unsuccessful in dis-

tinguishing adequately which characters were, in fact, hereditary or in providing evidence of the origin of new characters or a mechanism for sustained evolution. The espousal of neo-Darwinism by Weismann's school eventually was a factor in the decline of Darwinism, instead of leading to its general adoption.

The 19th century ended with naturalists and other students of evolution in quite fundamental disagreement. Various forms of modified Darwinism were still dominant, but they were bitterly opposed by some able students and had been subjected to criticisms for which no adequate answer had been found. Progress in the field of general evolutionary theory demanded more knowledge of heredity and, from this time, the students of that subject began to play a leading role. Their influence, at first, was far from unifying. Instead of producing evidence in favor of one or another of the competing types of evolutionary theory, they split the field still farther. De Vries, who published his mutation theory in 1901,[3] showed that distinctive types of plants may arise all at once and at random and he generalized this as the mooted normal course of evolution, a theory consistent neither with Darwinism nor with Lamarckism in any of their diverse forms. As Mendelism was rediscovered and the modern science of genetics developed, de Vries' concepts turned out to be limited and even a little crude, but the evidence of genetics still seemed to many to favor the origin of new forms of life at random, without reference to the environment, and without any subsequent adaptation under the influence of selection.

The *coup de grace* seemed to have been given to Darwinism and neo-Darwinism. A host of alternative theories, old and new, found support in what sometimes was like a battle-royal among the evolutionists. Varied as these theories are, most of them can be classed in four broad schools:

1. *Neo-Lamarckism* continued to have a few adherents. They emphasized the fact (particularly evident to paleontologists) that progressive adaptation does occur, but they agreed with the criticism that natural selection is not an adequate explanation of this phenomenon. Cope, Hertwig, Kammerer, and McDougall exemplify students of different but generally Lamarckian shades of opinion, over a considerable period. Despite their efforts, they failed to provide reasonable neo-Lamarckian explanations of many of the phenomena of evolution. Especially, they failed to controvert the growing genetical evidence

that neo-Lamarckism is impossible, because modifications (as opposed to mutations) cannot be inherited as such, if at all.

2. The many theories that may be classed broadly as vitalistic (in some cases, but misleadingly styled "orthogenetic") assume that evolution follows well-defined lines that are not mechanistically determined but are set by some inner cause or non-material force. Bergson's "*élan vital,*" Driesch's "entelechy," Osborn's "aristogenesis," Berg's "nomogenesis," and Broom's evolution directed by spiritual agencies are well-known and, in their various ways, typical. Such theories are essentially non- and sometimes even anti-genetic. They either assume that the directive force operates by producing directional (non-random) mutation, or that mutation is a mere accident having little to do with evolution in the long run. Opponents of these views object to them on the grounds that they are not science but metaphysics or philosophy, and that they are explanations that do not explain, examples of the naming fallacy. As J. S. Huxley has remarked, saying that progressive evolution is caused by an *élan vital* is like "explaining" the motion of a railroad train by an *élan locomotif*.

3. A small and not very popular class of theories is exemplified by Lotsy's evolution by means of hybridization[4] and by Clark's "zoogenesis."[5] Different as these theories are, they have in common that they practically deny the reality of evolution, if we assume that evolution necessarily includes the appearance of new characters from time to time. Lotsy's theory involves little more than the reshuffling of genes and Clark's is essentially based on the sudden segregation of existing genetic characters. Critics of these theories grant that these are unquestionably processes that do occur in evolution, but insist that attempting a general explanation of evolution on such a basis is begging the question. It is necessary to explain how the genes originated in the first place, how they came to be distributed as they were before shuffling and segregation, and what the mechanism of these redistributions is.

4. Finally, there is a group of theories more important than those already listed and stemming from de Vries and the early geneticists. De Vries called his theory "mutation," but this is now misleading, because the word mutation has come to have quite a different meaning. It might be called the saltation or the catastrophic theory, although the leading modern exponent of a theory of this type (Goldschmidt)[6] particularly objects to the latter label. Other geneticists, without nec-

essarily believing that species arise all at once by one big "systemic mutation," have held essentially similar views, maintaining that new variants (or new species) arise spontaneously, at random, and then live, if they can, or die, if they must. These students believe in pre-adaptation but not in adaptation. They believe that selection eliminates the monstrously unfit, but has no further bearing on evolution.

It was theories of this fourth class that were claimed to have put an end both to Darwinism and to Lamarckism. Yet their proponents ignored a vast amount of evidence, accumulated especially by the paleontologists, that seemed quite inexplicable by the current genetic theories. The result, culminating in the early '20's, was hopeless disagreement among paleontologists, geneticists, and systematists (heirs of the old naturalists), the three main groups of students of evolutionary theory. Nor were the students in any one of these groups in agreement among themselves on any fundamental point, except that evolution has somehow managed to occur.

Paleontology and Evolutionary Theory

It seems obvious to us that evolution is the only rational explanation of the paleontological record and that that record is the most direct and unmistakable exemplification of evolution. It seems paradoxical that the paleontologists were not the first to observe this. The fact is that early paleontologists regularly cited the fossil record as evidence *against* the reality of evolution. It was not until Darwin had overwhelmingly established the probability of evolution, with no real help from the paleontologists, that they accepted this as the great unifying principle in their science and began systematically to produce evidence in its favor. Even so, Owen, one of the most eminent of paleontologists, lived for 33 years after the publication of "The Origin of Species," without ever accepting the reality of evolution, still less of Darwin's explanation of it. Meanwhile, however, men like Kowalevsky, in Europe, and Cope and Marsh, in America, had produced conclusive paleontological evidence that evolution is a fact.

This simple but laborious and necessary demonstration was the first and remains one of the greatest contributions of paleontology to evolutionary theory. The function is almost entirely descriptive, and it must be admitted that the tradition of treating paleontology as a descriptive science is still strong. Even now, relatively few paleontolo-

gists are contributing to theory above the descriptive level. This is less true in Europe than in America, where (although it may surprise some to think of practicality in connection with paleontology) paleontology is pursued as much for practical as for truly scientific reasons. It is also less true of vertebrate than of invertebrate paleontology, which has developed more as a geological than as a biological science.

Most of the paleontological theories that will be found in books on evolution or on fossils are really generalized descriptions and not theories in any other sense of the word. For instance, Dollo's so-called law of irreversibility is properly no more than a description of what has been found by observation to be usual in the fossil record. Dollo made no serious attempt to relate this generalization to the evolutionary factors that must be involved. When such an attempt at higher or more truly theoretical study of these phenomena was belatedly made, Dollo's "law" had to be profoundly modified. Similarly, Osborn's adaptive radiation, Gregory's isomerism, and even orthogenesis, as paleontologists have commonly used the word, are really descriptions of what usually or sometimes happened, and not attempts to explain these phenomena in terms of broader evolutionary principles. These descriptive generalizations are extremely important and interesting and have a great contribution to make to the study of evolution and of life, but this contribution cannot well be realized until they are more closely related to the results of genetics and other branches of neobiology. On the other hand, the neobiologists have been deprived of data of crucial importance in their studies, because they have not themselves (with very rare exceptions) understood the bearing of these paleontological generalizations in the broader fields of theory.

Of course, there have always been some paleontologists who were competent and interested students of evolution, in addition to being observers of the course of evolution. Even so, it is rather striking how often they seem to have been working at cross-purposes with the neobiologists—I do not mean to say which group, if either, was at fault. Thus, Cope, who still stands out as the most soundly philosophical paleontologist that America has produced, was a confirmed neo-Lamarckian, and this throughout the period of the first strong enthusiasm for Darwinism and even, at the end of Cope's life, into the period of violently anti-Lamarckian Weismannian neo-Darwinism. Neo-Lamarckism has always had more adherents among paleontologists than among neobiol-

ogists and, to this day, there is a neo-Lamarckian tinge in much paleontological writing. This viewpoint has been so thoroughly undermined by the geneticists that it can barely be called intellectually respectable, at present, and yet the paleontologist, if so inclined, can justly point out that the facts that suggest neo-Lamarckism to him have been ignored by the geneticists.

Growing divergence between paleontologists and neobiologists became acute or apparently complete when the geneticists first came into the ascendency as students of evolution. As has been seen, the main early contribution of the experimental study of heredity to evolutionary theory was mutation in the sense of de Vries. But, in the meantime, paleontologists had produced several descriptive generalizations, apparently inexplicable by de Vriesian mutation or similar later theories of the geneticists, and nevertheless demanding explanation in any acceptable general theory of evolution. One of these generalizations was that evolution is gradual. In the nature of things, this can never be observed in all cases, but even in the '90's it had been found so frequently that most paleontologists believed then, as they do now, that this is usual, if not universal. This is consistent with either Lamarckism or Darwinism. It is not consistent with de Vriesian mutation, at least as a general pattern of evolution, and so it was instrumental in the widespread rejection of that theory and its successors by paleontologists.

Another major generalization involved in the rift between paleontology and genetics was that evolution is often, perhaps usually, sustained and directional. This, too, has been considered inconsistent with de Vriesian or Mendelian mutation, which seems to be essentially random and to provide no mechanism for prolonged, gradual change in a single direction. Some paleontologists considered the phenomenon consistent with Darwinism ("orthoselection") and some did not. On the whole, it seems to have weighed against the complete acceptance of Darwinism, by paleontologists. It is not inconsistent with neo-Lamarckism. Indeed, Plate[7] defined orthogenesis as caused by the direct influence of the environment.

Paleontologists were and are also very insistent that progressive adaptation is typical of evolution without necessarily being universal. Adaptive radiation is one of many paleontological generalizations bearing on this point. Adaptation is one of the leading features of Dar-

winian theory and their observations on this point were instrumental in the fact that so many paleontologists were Darwinists even in the period when Darwinism's demise had been widely announced. Until recently, most geneticists were anti-Darwinian on this point, and this was another reason why paleontologists and geneticists could find no common ground. Like directional evolution, adaptation is consistent with Lamarckism, but, for other reasons, neo-Lamarckism was being abandoned even by paleontologists. Most of them recognized that the geneticists had disproved the inheritance of modifications, even though they could not see that the geneticists had any suitable substitute to offer.

Paleontologists who wished to push their theoretical enquiries beyond the important but relatively superficial level of descriptive generalization were thus in a serious dilemma. None of the three current mechanistic schools, those typified by the names of Lamarck, Darwin, and de Vries, seemed fully adequate to interpret paleontological observations, and the paleontologists themselves failed to find a satisfying alternative. It was in this situation that some paleontologists began to support a non-mechanistic, that is, in a broad sense, a vitalistic explanation. The outstanding example is Osborn's theory of evolution characterized by the (supposed) phenomenon that he called successively definite variation, rectigradation, and aristogenesis. It was almost inevitable that such an escape from the dilemma would be sought by a deeply philosophical paleontologist of his period.

Osborn's theory was not adopted in its entirety or in his terms by any other student. This was, in part, due to the fact that most paleontologists were not sufficiently interested or instructed in biological theory to be aware of the reality and intensity of the dilemma and, in part, it was due to the obscurity and the personal terminology of Osborn's expositions of his views. Nevertheless, his espousal of a form of vitalism (which he did not call by that name) was more important and had more influence than has been generally recognized. Belief in some sort of non-mechanistic factor in evolution, often expressed only vaguely or by implication, became rather widespread among the more thoughtful paleontologists and is by no means negligible at the present time. Yet to most of us, paleontologists, geneticists, and systematists alike, no such theory is acceptable, any more than Bergson's *élan vital* was acceptable as science, and for the same reasons.

The failure of the geneticists to provide explanations really applic-

able to some of the paleontological data and the failure of the paleontologists to find for themselves a really satisfactory way out of their dilemma led to mistrust and even to open antagonism, as between Osborn and Morgan, in one period of their work. The geneticists tended to consider that paleontology was incapable of rising above pure description and they did not even take the trouble to study descriptive paleontology for its bearing on genetics. It was easier to conclude that it had no such bearing. The paleontologists were, as a rule, quite willing to accept this stultifying conclusion, which also spared them the trouble of learning genetics. Many of them felt that genetics was a milk-bottle science, with little or no bearing on evolution in the large, and if this attitude now seems ridiculously provincial to us, it must be admitted that the geneticists did nothing to change it until quite recently.

The position of the systematists was, if anything, still less happy. It was their predecessors who discovered evolution. Darwin's demonstration of the reality of evolution and his particular theory as to how evolution occurs had been based almost entirely on work in fields now included in systematics. The workers of the subsequent three decades who so actively discussed the subject and who seemed for a time about to carry the field for Darwinism were also systematists, for the most part. But it was also the systematists who led the first scientific attacks on Darwinism and who accelerated its decline by helping to reveal its real and great defects. As an example, Robson's work comes immediately to mind. The period of the rise of genetics and decline of Darwinism was also one of decline for systematics The science became unpopular and the systematists were even despised by many geneticists and other experimental biologists.

Involved in a dilemma even more pressing than that of the paleontologists, many of the surviving systematists simply ignored the new developments and carried on as if they were still in the 19th century, contributing thereby to the scorn heaped on their subject by more progressive students. Others, in the manner of Lotsy and Clark, tried to synthesize genetics with their data but produced only abortive, radically incomplete, or otherwise unsatisfactory solutions. Some, and they are the ones who now seem to have been justified, continued to support modified Darwinism, while broadening and modernizing their methods and lines of study in an attempt to find solutions to the difficulties that

had arisen. Many of them came to agree with Kellogg ("Darwinism Today," 1907)[8] that selection *must* work, even though it was not apparent how it does so.

Synthesis

The history of evolutionary theory, as followed in outline up to this point, is one of disillusion and growing confusion. When a science is advancing along sound lines, it is to be expected that its various avenues of enquiry will tend to converge as regards the more fundamental problems, however diverse may be opinions regarding details. Students of different aspects of evolution in the latter part of the 19th and first quarter of the 20th centuries seemed, on the contrary, to be diverging more and more, perhaps irreconcilably. So extreme and increasing was the conflict, that it was questionable whether all the work done after 1859 had really resulted in any real progress as regards the basic factors of evolution. There is some justification for Shull's suggestion ("Evolution," 1936)[9] that evolutionary theory would now be on a sounder basis if all speculation concerning it had been banned after publication of "The Origin of Species," until the 1920's or '30's.

Yet, in retrospect, it does not really appear that Shull is right on this point or that the efforts made between 1859 and 1920 were wasted. The great accumulation of concrete data has permanent validity and is indispensable. The intelligent gathering of those data required some theoretical aim, even when the data eventually proved that the theories were wrong. The Baconian system of first gathering all possible facts and then proceeding to inductive theorizing has long since been found sterile and is not, in truth, part of the scientific method as it actually works. Nor is it true that all the theories advanced during this period have had to be abandoned, or are likely to be.

It now appears that most of the students of evolution, in its various different aspects, were really converging toward the same fundamental conclusions, but that they were unaware of this because of the barriers between their different avenues of approach. Or, to change the figure of speech, it is as if each student had found a piece of a jigsaw picture, along with a piece or two that was not part of the same picture, and that each was so convinced that he had the essential pieces that they never got together and pooled their pieces to see what the whole picture was really like, to learn what pieces did not fit in, and to determine

what pieces were still missing and must be the objects of further search.

Even in the '20's, a pattern and a consensus were beginning to emerge from the welter of facts and theories that had so long been piling up. One symptom was the swing of the geneticists toward Darwinism, as soon as the fundamentals of individual inheritance were fairly well in hand and the geneticists began to think in terms of gene systems, and, especially, of populations rather than of single genes and isolated lines. For instance, Morgan, dean of geneticists, was an outspoken opponent of Darwinism earlier in his career, but had become definitely neo-Darwinian by 1925 (when he published "Evolution and Genetics").[10] It was not long before geneticists had taken over the defense of Darwinism from the systematists and were assisting in the synthesis of genetics and systematics. A feature of this movement is the mathematical treatment of population genetics, by Fisher,[11] Haldane,[12] Wright,[13] and others. Their work has reinstated natural selection as an effective evolutionary agency. It has finally solved the major problem of how the adaptive results observed by the systematists and paleontologists could be achieved with the hereditary materials and mechanisms observed by the geneticists. An overwhelming mass of genetical data has been marshalled and interpreted in neo-Darwinian terms by Dobzhansky in his great work "Genetics and the Origin of Species" (1937, 1941).[14]

The systematists found their field in favor again. They found, too, that they were no longer at cross-purposes with the geneticists, but were able to work at the same problems with the great force of the combined tools of these two branches of science. "The New Systematics" (1940, edited by J. S. Huxley[15]) illustrated and accelerated this hopeful trend. Mayr's "Systematics and the Origin of Species" (1942)[16] did for the systematists what Dobzhansky's book had done for the geneticists, as the intentional similarity of title suggests. J. S. Huxley provided a balanced treatment from both points of view in "Evolution, the Modern Synthesis" (1942).[17] This epochal book is crowded with examples (in the Darwinian tradition) and it fully, ably, and convincingly presents the neo-Darwinian position as newly reached by geneticists and systematists in unison. It stands as a landmark for a new era of evolutionary theory.

The name "neo-Darwinism" for this recent development is not altogether happy. It is a far cry from Darwinism to this modern theory.

As a matter of fact, it is a still farther cry from the neo-Darwinism of the turn of the century, to what Huxley and others are now calling neo-Darwinism. Yet, the new neo-Darwinism—perhaps, should one say neo-neo-Darwinism—has evolved from Darwin's original and complex theory, and it is not inappropriate that it should perpetuate his name.

Besides adding a wealth of detail, the theory differs essentially from that of Darwin in identifying the truly hereditary units, in accepting the evidence that they are particulate and not continuous, and in recognizing that they do not directly represent or correspond with somatic characters. Strictly Lamarckian factors are rejected. Vitalism in all its guises and disguises is also excluded, not necessarily as being false but as being outside the field of scientific enquiry and not required to explain the facts of evolution, at least on the material level. The Lotsian factor of hybridization and the various other factors involving shuffling and segregation of genes are recognized as parts of the evolutionary process, but not particularly the most essential parts and not, certainly, the whole process. Pre-adaptation is accepted as a fact and as another of the multiple factors of evolution, but, again, it is not considered to be the leading element in evolution. Adaptation, in the Darwinian sense, or post-adaptation, under the influence of natural selection, is held to be an important and widespread phenomenon. Another, more important difference from the original mutation theory, either as proposed by de Vries or in the modern form supported by Goldschmidt, is the belief that small mutations have, on the whole, been more effective than large mutations and, especially, that groups with essentially new genetic systems usually arise gradually under the directive influence of selection and not all at once or at random. Selection is emphasized as an essential, but, of course, not as the only factor in evolution, and it is given a truly creative role. It determines, on the whole and with certain fairly well-defined exceptions, not only what particular mutations will survive, but also what particular combinations of genes or genetic systems and, hence, what somatic types, will be realized.

It would not be expected and it is not the fact that all students of this neo-Darwinian school agree in all respects, nor that all students of evolution now adhere to this school. The degree of agreement and the extent of the consensus are nevertheless quite remarkable. In England and America, only one outstanding and thorough-going dissenter has so

far raised his voice: Goldschmidt.[6] It remains to be seen how rapidly and how generally this movement will spread elsewhere. It reached its full formulation only immediately before and during the war and the reaction of, for instance, the French, German, and Italian students is not yet evident. Non- and anti-Darwinian theories, in great variety, were rather widely held among these students, but it is not unlikely that they will find the new synthesis as useful as have so many English and American (one may add also Russian) students.

So far, this is an impressive and apparently most satisfactory outcome, but, in fact, the synthesis as presented up to this point is not really satisfactory. It is decidedly incomplete and it still has fundamental weakness, because it has not taken adequate notice of the paleontological data and theories. Several of the recent neo-Darwinian works, already referred to, mention paleontology in passing, but they do not derive much from it. Sometimes, the attempt is made to explain away inconvenient paleontological descriptive principles, and sometimes these are mentioned with an admission that they are inexplicable by neo-Darwinism. No theory of evolution can long be satisfactory, even to the geneticists and the systematists, unless it is explicitly shown to be harmonious with the factual record of evolution as revealed by paleontology. Moreover, there are very essential parts of a general theory of evolution that cannot be based on the study of recent animals and plants, alone.

This weakness is revealed, among other indications, by the repetition of the classic Darwinian expression, "the origin of species," in the titles of the books by Dobzhansky and Mayr. They are, of necessity, dealing with the lowest taxonomic levels and with the smallest steps in the evolutionary scale. This is indubitably an excellent point of attack, indeed the only good point when paleontology is left out of the picture, but it does not suffice for a thorough synthesis. There may be some *a priori* likelihood that the factors of evolution involved in large-scale evolution, effecting major morphological changes, and extending over millions of years, will be the same as those involved in speciation, but this can by no means be taken as a firm assumption. It is not even likely that those factors will combine in the same way and produce identical patterns or modes in the larger picture as in the smaller.

Until these points and others related to them have been restudied with the aid of paleontology, the theory cannot be well established. It

is even possible to argue, as Goldschmidt has done so fluently and forcefully, that the neo-Darwinians are amusing themselves with mere minor fluctuations and that all the evolutionary changes that have really mattered have depended on totally different principles.

It is not only that the theory is incomplete until it is supplemented by the inclusion of paleontology in the synthesis, but also that it may be quite wrong, even on its lowest levels, as long as there are apparent inconsistencies between theory and what the fossil record suggests as fact. Both Haldane and Huxley, two of the leading neo-Darwinians, indicate that such inconsistencies may still exist. It is thus not unlikely that their partial synthesis will have to be not only expanded but also altered when it becomes a complete synthesis of evolutionary studies.

The testing of the neo-Darwinian synthesis and, at least, a first step toward its enlargement and modification by the inclusion of paleontology were two of the problems that I set for myself in "Tempo and Mode in Evolution." My proposal for this paper to show how such a study became necessary and what place it has in the development of evolutionary theory has now been carried out. I do not propose to go into any detail as to the results of that study, and those who have read the book know what results were there reached, and also that these results are tentative and quite incomplete. It would, however, be disappointing to conclude this essentially historical study without any mention of the present position or the possible future.

It is my belief that neo-Darwinism is capable of resolving the paleontologists' dilemma to which I have referred. With some modification in detail, partly to be contributed by paleontology, I do not find the neo-Darwinian theory inconsistent with any evolutionary phenomenon as revealed in the fossil record and known to me. It is, to be sure, a long way from mere lack of inconsistency to anything approaching an acceptable scientific explanation. On many points, neo-Darwinism, in its present stage, does provide such an explanation. On others, it does not, but it seems to come as close to this goal as any alternative yet proposed. The further development of this theory seems to me likely to come as close as we are ever going to come to a really general explanation of evolution. It is, of course, inevitable that the theory itself will evolve and it may even change almost completely as more and more is added to the synthesis. The essential and hopeful point is that

paleontologists, geneticists, and systematists have found a common ground at last, that a synthesis is seen as a real possibility, and that all three types of students, and others, are interested in this aim and are actively working toward it together.

LITERATURE CITED

1. **Simpson, George Gaylord**
   1944. Tempo and Mode in Evolution. Columbia University Press. New York
2. **Weismann, August**
   1892. Das Keimplasma, eine Theorie der Vererbung. Gustav Fischer. Jena.
3. **De Vries, Hugo**
   1901. Die Mutationstheorie. Veit. Leipzig.
4. **Lotsy, John P.**
   1916. Evolution by Means of Hybridization. Martinus Nijhoff. The Hague.
5. **Clark, Austin H.**
   1930. The New Evolution—Zoogenesis. The William & William Co. Baltimore.
6. **Goldschmidt, Richard**
   1938. Physiological Genetics. McGraw-Hill Book Co. New York & London.
   1940. The Material Basis of Evolution. Yale University Press. New Haven.
7. **Plate, Ludwig H.**
   1913. Selektionsprinzip und Probleme der Artbildung. 4 Aufl. Verlag von Wilhelm Engelmann. Leipzig & Berlin.
8. **Kellog, Vernon L.**
   1907. Darwinism Today. Henry Holt & Co. New York.
9. **Shull, Aaron F.**
   1936. Evolution. 2nd ed. McGraw-Hill Book Co. New York.
10. **Morgan, Thomas Hunt**
    1925. Evolution and Genetics. Princeton University Press. Princeton.
11. **Fisher, R. A.**
    1930. The Genetical Theory of Natural Selection. Clarendon Press. Oxford.
12. **Haldane, John B. S.**
    1932. The Causes of Evolution. Harper & Bros. New York & London.
    1942. New Paths in Genetics. Harper & Bros. New York & London.
13. **Wright, Sewall**
    1931. Evolution in Mendelian Populations. Genetics **16**: 97–159.
    1932. The Roles of Mutation, Inbreeding, Crossbreeding, and Selection in Evolution. Proc. 6th Int. Cong. Genetics **1**: 356–366.
    1935. Evolution in Populations in Approximate Equilibrium. J. Genetics **30**: 257–266.
    1942. Statistical Genetics and Evolution. Bull. Amer. Math. So. **48**: 223–246.
14. **Dobzhansky, Theodosius**
    1937. Genetics and the Origin of Species. Columbia University Press. New York.
    1941. 2nd ed. of above.
15. **Huxley, Julian S.**
    1940. The New Systematics. The Clarendon Press. Oxford.

16. **Mayr, Ernst**
1942. Systematics and the Origin of Species. Columbia University Press. New York.

17. **Huxley, Julian S.**
1942. Evolution, the Modern Synthesis. George Allen & Unwin Ltd. London.

SECTION OF PSYCHOLOGY

NOVEMBER 19, 1945

DOCTOR GORDON W. ALLPORT and DOCTOR LEO J. POSTMAN,* Department of Psychology, Harvard University, Cambridge: *The Basic Psychology of Rumor.* (This lecture was illustrated by lantern slides.)

Although the disadvantages of war far outweigh its advantages, yet we may reckon among its meagre benefits the powerful incentives and exceptional opportunities that war gives to scientists to advance their knowledge in fields which normally they neglect to explore or are wont to explore in a desultory fashion. Social psychology is one of the sciences whose work has been greatly stimulated during the recent conflict. Under the stress of wartime needs, it has made significant progress in several areas of investigation. Among them, we name propaganda analysis, morale studies, public opinion measurement, food habits, group therapy, situational tests for selecting personnel, minority group problems, the nature of prejudice, of re-education, and of rumor.[1] It is the last area of progress that we shall here explore.

RUMORS IN WARTIME

During the year 1942, rumor became a national problem of considerable urgency. Its first dangerous manifestation was felt soon after the initial shock of Pearl Harbor. This traumatic event dislocated our normal channels of communication by bringing into existence an unfamiliar and unwelcome, if at the same time a relatively mild censorship of news, and it simultaneously dislocated the lives of millions of citizens whose futures abruptly became hostages to fortune.

This combination of circumstances created the most fertile of all possible soils for the propagation of rumor. We now know that *rumors concerning a given subject-matter will circulate within a group in pro-*

* This paper was presented by Dr. Allport.

[1] For a review of civilian wartime investigations in these fields, see **G. W. Allport & H. R. Veltfort** Social Psychology and the Civilian War Effort, J. Soc. Psychol. (S.P.S.S.I. Bulletin). **18:** 165–233, 1943; **G. R. Schmeidler & G. W. Allport** Social Psychology and the Civilian War Effort, May 1943–May 1944. J. Soc. Psychol. (S.P.S.S.I. Bulletin) **20:** 145–180. 1944.

portion to the importance and the ambiguity of this subject-matter in the lives of individual members of the group.

The affair of Pearl Harbor was fraught with both importance and ambiguity to nearly every citizen. The affair was important because of the potential danger it represented to all of us, and because its aftermath of mobilization affected every life. It was ambiguous because no one seemed quite certain of the extent of, reasons for, or consequences of the attack. Since the two conditions of rumor—importance and ambiguity—were at a maximum, we had an unprecedented flood of what became known as "Pearl Harbor rumors." It was said that our fleet was "wiped out," that Washington didn't dare to tell the extent of the damage, that Hawaii was in the hands of the Japanese. So widespread and so demoralizing were these tales that, on February 22, 1942, President Roosevelt broadcast a speech devoted entirely to denying the harmful rumors and to reiterating the official report on the losses.

Did the solemn assurance of the Commander-in-Chief restore the confidence of the people and eliminate the tales of suspicion and fear? It so happens that a bit of objective evidence on this question became available to us almost by accident. On the twenty-first of February, the day before the President's speech, we had asked approximately two-hundred college students whether they thought our losses at Pearl Harbor were "greater," "much greater," or "no greater" than the official Knox report had stated. Among these students, 68 per cent had believed the demoralizing rumors in preference to the official report, and insisted that the losses were "greater" or "much greater" than Washington admitted. Then came the President's speech. The next day, an equivalent group of college students were asked the same question. Among those who had not heard or read the speech the proportion of rumor-believers was still about two-thirds. But among those who were acquainted with the President's speech, the number of rumor-believers fell by 24 per cent. It is important to note that, in spite of the utmost efforts of the highest authority to allay anxiety, approximately 44 per cent of the college population studied were too profoundly affected by the event and by the resulting rumors to accept the reassurance.

The year 1942 was characterised by floods of similar fear-inspired tales. Shipping losses were fantastically exaggerated. Knapp records one instance where a collier was sunk through accident near the Cape Cod Canal. So great was the anxiety of the New England public that

this incident became a fantastic tale of an American ship being torpedoed with the loss of thousands of nurses who were aboard her.[2]

Such wild stories, as we have said, are due to the grave importance of the subject for the average citizen and to the ambiguity to him of the objective situation. This ambiguity may result from the failure of communications, or from a total lack of authentic news, a condition that often prevailed in war-torn countries or among isolated bands of troops who had few reliable sources of news. Again, the ambiguity may be due to the receipt of conflicting news stories, no one more credible than another; or it may be due (as in the case of the Pearl Harbor rumors) to the distrust of many people in the candor of the Administration and in the operation of wartime censorship. As the war progressed, a higher degree of confidence in our news services was rapidly achieved, and rumors concurrently subsided.

In addition to the fear-rumors of 1942, which persisted until the tide of victory commenced to turn, there was a still more numerous crop of hostility-rumors whose theme dealt always with the shortcomings, disloyalty, or inefficiency of some special group of co-belligerents. The Army, the Navy, the Administration, our allies, or American minority groups were the most frequent scapegoats in these rumors. We were told that the Army wasted whole sides of beef, that the Russians greased their guns with lend-lease butter, that Negroes were saving ice-picks for a revolt, and that Jews were evading the draft.

These hostility rumors were the most numerous of all. An analysis of 1000 rumors collected from all parts of the country in 1942,[3] revealed that they could be classified fairly readily as:

Hostility (wedge-driving) rumors	=	66 per cent
Fear (bogey) rumors	=	25 per cent
Wish (pipe-dream) rumors	=	2 per cent
Unclassifiable rumors	=	7 per cent
TOTAL		100 per cent

To be sure, the proportion of fear and wish rumors soon altered. As victory approached, especially on the eve of VE and VJ day, the whirlwind of rumors was almost wholly concerned with the cessation of hostilities, reflecting a goal-gradient phenomenon whereby rumor under special conditions hastens the completion of a desired event. But,

[2] R. H. Knapp A Psychology of Rumor. Pub. Op. Quart. 8: 22–37. 1944.
[3] R. H. Knapp op. cit.: 25.

throughout the war and continuing to the present, it is probably true that the majority of all rumors are of a more or less slanderous nature, expressing hostility against this group or that.

The principal reason why rumor circulates can be briefly stated. It circulates because it *serves the twin function of explaining and relieving emotional tensions felt by individuals.*[4]

The Pearl Harbor rumors, for example, helped to *explain* to the teller why he felt such distressing anxiety. Would his jitters not be justified if it were true that our protecting fleet was "wiped out" at Pearl Harbor? Something serious must have happened to account for his anxiety. Families deprived of sons, husbands or fathers, vaguely cast around for someone to blame for their privation. Well, the Jews, who were said to be evading the draft, were "obviously" not doing their share and thus the heavy burden falling on "good citizens" was explained. True, this draft-evasion charge did not last very long, owing, no doubt, to the inescapable evidence of heavy enlistments among Jews and of their heroic conduct in the war. But when shortages were felt, the traditional Jewish scapegoat was again trotted out as a convenient explanation of the privations suffered. Their operation of the black market "explained" our annoying experiences in the futile pursuit of an evening lamb-chop.

To blame others verbally is not only a mode of explanation for one's emotional distress, but is at the same time a mode of *relief.* Everyone knows the reduction of tension that comes after administering a tongue-lashing. It matters little whether the victim of the tongue-lashing is guilty or not. Dressing down *anyone* to his face or behind his back has the strange property of temporarily reducing hatred felt against this person or, what is more remarkable, of reducing hatred felt against any person or thing. If you wish to deflate a taut inner-tube you can unscrew the valve or you can make a puncture. Unscrewing the valve corresponds to directing our hostility toward the Nazis or Japanese, who were the cause of our suffering. Making a puncture corresponds to displacing the hostility upon innocent victims

[4] This brief formula leaves out of account only the relatively few rumors which seem to serve the purpose of "phatic communication,"—a form of idle conversation to facilitate social intercourse. When a lull occurs in a conversation, an individual may "fill in" with the latest bit of gossip that comes to mind, without being motivated by the deeper tensions that underlie the great bulk of rumor-mongering.

In this paper we cannot enter into a fuller discussion of the reasons why people believe some rumors and not others. This question is carefully studied by **F. H. Allport & M. Lepkin** Wartime Rumors of Waste and Special Privilege: Why Some People Believe Them, J. Abnorm. & Soc. Psychol. **40:** 3–36. **1945.**

or scapegoats. In either case, the air will escape and relaxation follow. To blame Jews, Negroes, the Administration, brass hats, the OPA, or the politicians, is to bring a certain relief from accumulated feelings of hostility whatever their true cause. Relief, odd as it may seem, comes also from "bogey" rumors. To tell my neighbor that the Cape Cod Canal is choked with corpses is an easy manner of projecting into the outer world my own choking anxieties concerning my son or my friends in combat service. Having shared my anxiety with my friend by telling him exaggerated tales of losses or of atrocities, I no longer feel so much alone and helpless. Through my rumor-spreading, others, too, are put "on the alert." I therefore feel reassured.

That rumors were harmful to national morale was quickly recognized both by federal authorities and by civilian leaders of opinion. The efforts of the FBI to trace subversive rumors constitute a story yet to be told; the preventive campaign conducted by OWI and other federal agencies marks another chapter in the story; the establishment of "Rumor Clinics" in at least 40 newspapers in the United States and Canada is yet another. Lectures, pamphlets, movies, posters, and "rumor-wardens" all formed part of the campaign. This activity was at its peak during 1942–43. As victory became assured, the emotional insistency of anxiety and hate subsided, news services became more widely believed, rumor lessened, and the immediate crisis passed.

Though it was the darker days of the war that focused our attention upon rumor as a grave social problem, still the mischief of rumor and gossip is something we always have with us. At the present time, there is reason to suppose that we may be headed for another critical period of rumor-mongering, since we anticipate sharp clashes between minority groups of Americans and majority groups during the coming years of social readjustment. Records of the bitter race conflicts in Los Angeles, Beaumont, Harlem, Philadelphia, and Detroit have taught us what a close association exists between rumors and riot. The tie is so intimate that one of the best barometers we have of social strain lies in the analysis of rumors circulating in a tense community.[5]

Experimental Approach

Leaving now the broader social setting of the problem, we ask

[5] For an account of the relation of rumors to riots see **A. McC. Lee & N. D. Humphrey** Race Riot. Dryden Press. New York. 1943; and **J. E. Weckler & T. E. Hall** The Police and Minority Groups. Internat. City Managers Association. Chicago. 1944.

ourselves what processes in the human mind account for the spectacular distortions and exaggerations that enter into the rumor-process, and lead to so much damage to the public intelligence and public conscience.

Since it is very difficult to trace in detail the course of a rumor in everyday life, we have endeavored by an experimental technique to study as many of the basic phenomena as possible under relatively well controlled laboratory conditions.

Our method is simple. A slide is thrown upon a screen. Ordinarily, a semi-dramatic picture is used containing a large number of related details. Six or seven subjects, who have not seen the picture, wait in an adjacent room. One of them enters and takes a position where he cannot see the screen. Someone in the audience (or the experimenter) describes the picture, giving about twenty details in the account. A second subject enters the room and stands beside the first

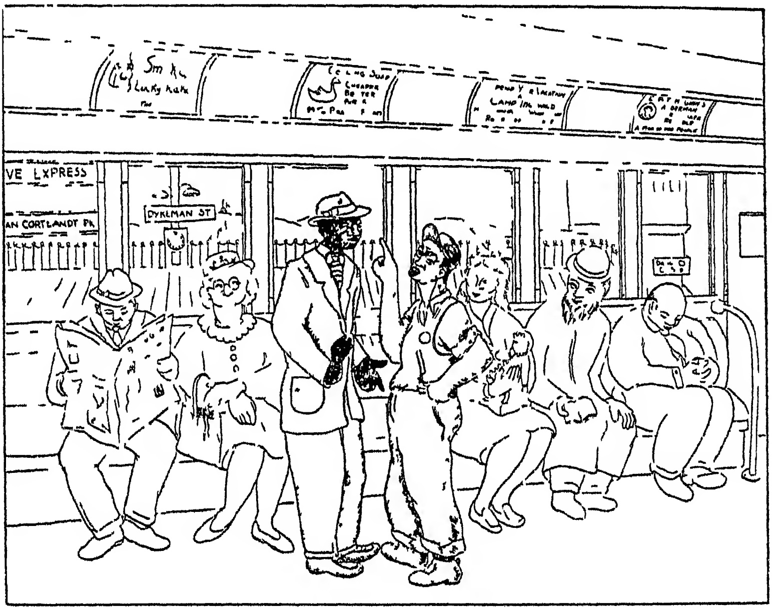

FIGURE 1. A sample of pictorial material employed in the experiments.—When the experiment was conducted at the New York Academy of Sciences, the terminal (sixth) report ran as follows:

> "A subway scene on the IRT, between Van Cortlandt Park and Dyckman Street. Four people are standing, two are seated. There is a colored man and a white man. One of them has a razor." (In the ante-terminal report, it was said that the Negro held the razor.)

subject who proceeds to tell him all he can about the picture. (All subjects are under instruction to report as "accurately as possible what you have heard.") The first subject then takes his seat, and a third enters to hear the story from the second subject. Each succeeding subject hears and repeats the story in the same way. Thus, the audience is able to watch the deterioration of the rumor by comparing the successive versions with the stimulus-picture which remains on the screen throughout the experiment.

This procedure has been used with over forty groups of subjects, including college undergraduates, Army trainees in ASTP, members of community forums, patients in an Army hospital, members of a Teachers' Round Table, and police officials in a training course. In addition to these adult subjects, children in a private school were used, in grades from the fourth through the ninth. In some experiments, Negro subjects took part along with whites, a fact which, as we shall see, had important consequences when the test-pictures depicted scenes with a "racial angle."

All of these experiments took place before an audience (20–300 spectators). By using volunteer subjects, one eliminates the danger of stage fright. There was, however, a social influence in all the audience situations. The magnitude of this influence was studied in a control group of experiments where no one was present in the room excepting the subject and the experimenter.

At the outset, it is necessary to admit that in five respects this experimental situation fails to reproduce accurately the conditions of rumor-spreading in everyday life. (1) The effect of an audience is considerable, tending to create caution and to shorten the report Without an audience subjects gave on the average twice as many details as with an audience. (2) The effect of the instructions is to maximize accuracy and induce caution. In ordinary rumor-spreading, there is no critical experimenter on hand to see whether the tale is rightly repeated. (3) There is no opportunity for subjects to ask questions of his informer. In ordinary rumor-spreading, the listener can chat with his informer and, if he wishes, cross-examine him. (4) The lapse of time between hearing and telling in the experimental situation is very slight. In ordinary rumor spreading, it is much greater. (5) Most important of all, the conditions of motivation are quite different. In the experiment, the subject is striving for *accuracy*.

His own fears, hates, wishes are not likely to be aroused under the experimental conditions. In short, he is not the spontaneous rumor-agent that he is in ordinary life. His stake in spreading the experimental rumor is neither personal nor deeply motivated.

It should be noted that all of these conditions, excepting the third, may be expected to enhance the accuracy of the report in the experimental situation, and to yield far less distortion and projection than in real-life rumor-spreading.

In spite of the fact that our experiment does not completely reproduce the normal conditions for rumor, still we believe that all essential changes and distortions are represented in our results. "Indoor" rumors may not be as lively, as emotionally-toned, or as extreme as "outdoor" rumors, and yet the same basic phenomena are demonstrable in both.

What happens in both real-life and laboratory rumors is a complex course of distortion in which three inter-related tendencies are clearly distinguishable.

Leveling

As rumor travels, it tends to grow shorter, more concise, more easily grasped and told. In successive versions, fewer words are used and fewer details are mentioned.

The number of details *retained* declines most sharply at the beginning of the series of reproductions. The number continues to decline, more slowly, throughout the experiment. Figure 2 shows the percentage of the details initially given which are retained in each successive reproduction.

The number of items enumerated in the description from the screen constitutes the 100 per cent level, and all subsequent percentages are calculated from that base. The curve, based on 11 experiments, shows that about 70 per cent of the details are eliminated in the course of five or six mouth-to-mouth transmissions, even when virtually no time lapse intervenes.

The curve is like the famous Ebbinghaus curve for decline in individual retention, though in his experiments the interval between initial learning and successive reproductions was not as short as under the conditions of our experiment. Comparing the present curve with Ebbinghaus's, we conclude that *social memory accomplishes as much*

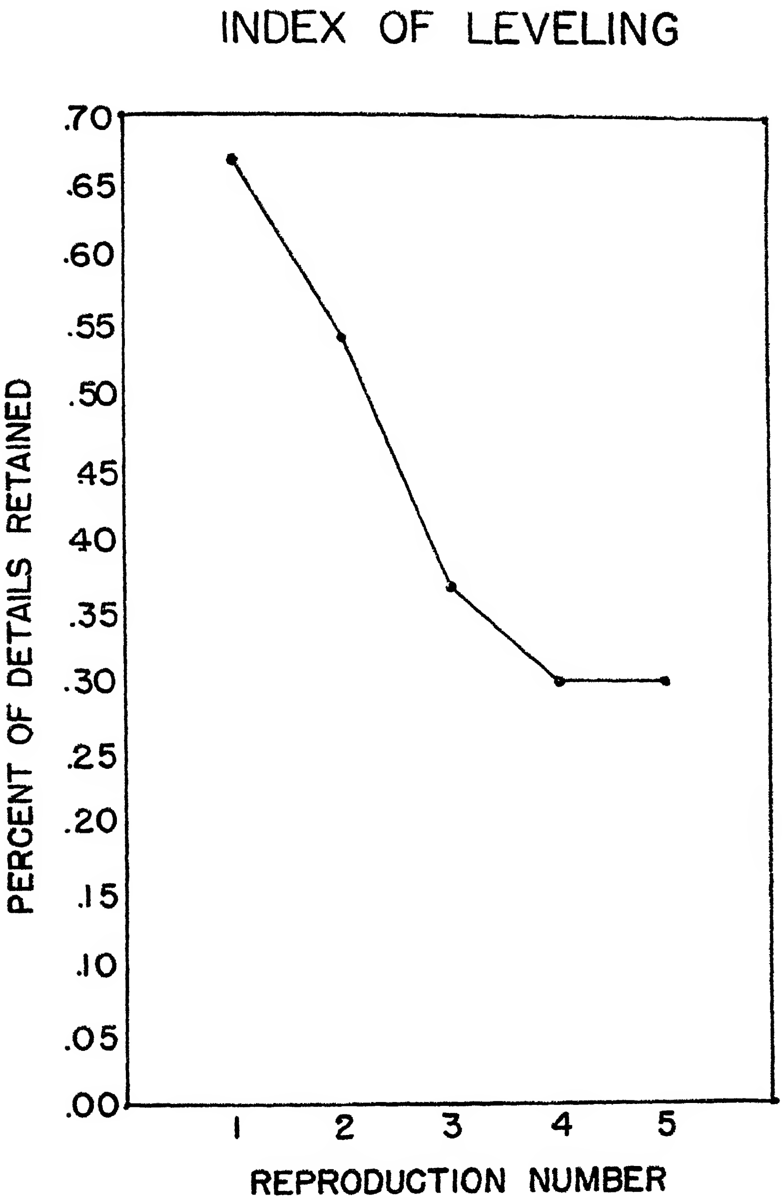

FIGURE 2. Percentage of details originally given which are retained in each successive reproduction.

leveling within a few minutes as individual memory accomplishes in weeks of time.

Leveling (in our experiments) never proceeds to the point of total obliteration. The stabilization of the last part of the curve is a finding of some consequence. It indicates (1) that a short concise statement is likely to be faithfully reproduced; (2) that when the report has become short and concise, the subject has very little detail to select from and the possibilities of further distortion grow fewer; (3) that the assignment becomes so easy that a virtually rote memory serves to hold the material in mind. In all cases, the terminal and the anteterminal reports are more similar than any two preceding reports.

The reliance on rote is probably more conspicuous in our experiments than in ordinary rumor-spreading, where accuracy is not the aim, where time interval interferes with rote retention, and where strong interests prevent literal memory. There are, however, conditions where rote memory plays a part in ordinary rumor-spreading. If the individual is motivated by no stronger desire than to make conversation, he may find himself idly repeating what he has recently heard in the form in which he heard it. If a rumor has become so crisp and brief, so sloganized, that it requires no effort to retain it in the literal form in which it was heard, rote memory seems to be involved. For example:

The Jews are evading the draft
The CIO is communist controlled
Wallace believes in a pint of milk for every Hottentot

The importance of rote has been recognized by the writers of advertisements. They endeavor to make their slogans brief, concise, rhythmic and easy to remember:

Lucky Strikes mean finer tobacco
Smoke Chesterfields—they satisfy
Duz does everything

Similarly, many legends and superstitions have been abbreviated to such an aphoristic point that it is almost impossible to forget them:

Stuff a cold and starve a fever
An apple a day keeps the doctor away
A red sky at night, the sailor's delight
Spare the rod and spoil the child
Early to bed and early to rise makes a man healthy and wealthy and wise

We conclude that whenever verbal material is transmitted among a group of people whether as rumor, legend, or history, change will be in the direction of greater brevity and conciseness. Leveling is not a random phenomenon—we note in the following protocol how a group of soldier subjects tends to retain a military orientation throughout their series of reports.

Protocol A

Description from the screen: The scene is laid in France during wartime. Several men in uniform are obvious. Two of them are firing, one is on his back wounded, with a bandage around his knee. There is a Negro soldier standing, ready to throw a hand-grenade. Behind them there is a destroyed building with one doorway. There is a sign at the crossroads, reading "Cherbourg 21½ km., Paris 50 km." There is also a sign reading "Pain et Vin." There are shells at the sides of the wrecked building. Behind the building there is a church with a big roof hole. The church has a steeple, with the clock showing ten minutes to two. There are two aeroplanes behind the church, as there are explosions to be seen. There is an ambulance at the extreme right, with men coming out with shells. Sign, "Bread and Wine."
First Reproduction: The scene is laid in France. There are two soldiers in a trench, close behind them, is another, wounded. Nearby there is a wrecked house. A Negro soldier is throwing a grenade. There are signs reading "50 miles to Cherbourg and 21 miles to Paris." There is a church with a steeple, showing ten minutes to two. The designation of shells bursting indicates that there is a battle going on. There is an ambulance somewhere in the picture. There is a sign "Bread and Wine."
Second Reproduction: The scene is in France. There is a trench with two men, one firing. A soldier is on his back, wounded. There is a signpost—"Paris 50 miles and Cherbourg 21 miles." There is an ambulance in the picture. There is a house or a barn behind a Negro soldier throwing a grenade. Behind the house is a church. On the steeple the time reads ten minutes to two. Behind the church there are some aeroplanes.
Third Reproduction: The scene is in France. There are two soldiers in a trench and a wounded soldier. There is an ambulance in the picture, and a house in the background, also a church with a steeple; the time is . . . I don't remember. There is a signpost "Cherbourg 21 miles, Paris 50 miles." There is a Negro soldier in the picture.
Fourth Reproduction: The scene takes place in France, 21 miles from Cherbourg, 50 miles from Paris. This information is given by a signpost. There are two soldiers in the picture and also a Negro soldier. In the distance there is a church, and also a house. There is an ambulance nearby.
Fifth Reproduction: The scene is in France, 21 miles from Cherbourg, 50 miles from Paris, as we can read on a signpost. There is a Negro soldier in the picture. There is a church nearby and also an ambulance.
Sixth Reproduction: The scene is in France, 21 miles from Cherbourg, and 50 miles from Paris, as a signpost indicates. There is a Negro soldier in the scene. An ambulance and a church are nearby.
Seventh Reproduction: The scene is in France, 21 miles from Cherbourg, 50 miles from Paris. There is a Negro soldier in the scene, and also an ambulance.
Eighth Reproduction: The scene is in France, 50 miles from Cherbourg and at a distance from Paris, and in this scene is an ambulance and also a Negro soldier.

This protocol shows the continual shortening of the rumor, but,

at the same time, the tendency of military subjects to preserve their orientation in space. The scene is always correctly laid in France, somewhere between Cherbourg and Paris. To be sure, kilometers are transposed into the more familiar measure of "miles," and the figure "50" gets attached to Cherbourg rather than Paris. Like every other rumor the report as received from hearsay is worthless, yet there is a selective type of retention that follows the occupational interest of the subject. Non-military subjects are much less likely to retain measures of distance or of time.

Sharpening

We may define sharpening as the selective perception, retention, and reporting of a limited number of details from a larger context. In the military protocol just cited, geographical features are sharpened. Sharpening is inevitably the reciprocal of leveling. The one cannot exist without the other, for what little remains to a rumor after leveling has taken place is by contrast unavoidably featured.

Although sharpening occurs in every protocol, the same items are not always emphasized. Sometimes, a trifling detail such as subway advertising card becomes the focus of attention and report. Around it the whole rumor becomes structured. But, in most experiments, this same detail drops out promptly, and is never heard of after the first reproduction.

One way in which sharpening seems to be determined is through the retention of odd, or attention-getting words which, having appeared early in the series, catch the attention of each successive listener and are often passed on in preference to other details intrinsically more important to the story. An instance of this effect is seen in a series of protocols where the statement, "there is a boy stealing and a man remonstrating with him" is transmitted throughout the entire series. The unusual word "remonstrate" somehow caught the attention of each successive listener and was passed on without change.

Sharpening may also take a *numerical* turn, as in the experiments where emphasized items become reduplicated in the telling. For example, in reports of a picture containing the figure of a Negro, whose size and unusual appearance invite emphasis, we find that the number of Negroes reported in the picture jumps from one to "four" or "several."

There is also *temporal* sharpening manifested in the tendency to

describe events as occurring in the immediate present. What happens *here* and *now* is of greatest interest and importance to the perceiver. In most instances, to be sure, the story is started in the present tense, but even when the initial description is couched in the past tense, immediate reversal occurs and the scene is contemporized by the listener. Obviously, this effect cannot occur in rumors which deal specifically with some alleged past (or future) event. One cannot contemporize the rumor that "the Queen Mary sailed this morning (or will sail tomorrow) with 10,000 troops aboard." Yet it not infrequently happens that stories gain in sharpening by tying them to present conditions. For example, a statement that Mr. X bought a chicken in the black market last week and paid $1.50 a pound for it may be (and usually is) rendered, "I hear they *are* charging $1.50 a pound on the black market for chicken." People are more interested in today than in last week, and the temptation, therefore, is to adapt (assimilate) the time of occurrence, when possible, to this interest.

Sharpening often takes place when there is a clear implication of *movement.* The flying of airplanes and the bursting of bombs are frequently stressed in the telling. Similarly, the falling flower pot in one picture is often retained and accented. Indeed, the "falling motif" may be extended to other objects such as the cigar which a man in the picture is smoking. In one rumor, it is said to be falling (like the flower pot), though in reality it is quite securely held between his teeth

Sometimes sharpening is achieved by ascribing movement to objects which are really stationary. Thus, a subway train, clearly at a standstill at a subway station, is frequently described as moving.

Relative size is also a primary determinant of attention. Objects that are prominent because of their size tend to be retained and sharpened. The first reporter calls attention to their prominence and each successive listener receives an impression of their largeness. He then proceeds to sharpen this impression in his memory. The large Negro may, in the telling, become "four Negroes," or may become "a gigantic statue of a Negro."

There are verbal as well as physical determinants of attention. Thus, there is a pronounced tendency for *labels* to persist, especially if they serve to set the stage for the story. One picture is usually introduced by some version of the statement, "This is a battle scene," and this label persists throughout the series of reproductions. Another

story usually opens with the statement, "This is a picture of a race riot."

To explain this type of sharpening, we may invoke the desire of the subject to achieve some spatial and temporal schema for the story to come. Such orientation is essential in ordinary life and appears to constitute a strong need even when imaginal material is dealt with.

An additional factor making for preferential retention of spatial and temporal labels is the *primacy* effect. An item that comes first in a series is likely to be better remembered than subsequent items. Usually, the "label" indicating place and time comes at the beginning of a report and thus benefits by the primacy effect.

Sharpening also occurs in relation to familiar symbols. In one series of reports, a church and a cross are among the most frequently reported items, although they are relatively minor details in the original picture. These well known symbols "pack" meaning and are familiar to all. The subject feels secure in reporting them because they have an accustomed concreteness that the other details in the picture lack. Retention of familiar symbols advances the process of conventionalization that is so prominent an aspect of rumor-embedding. In two of our pictures are a night stick, symbol of police authority, and a razor, stereotyped symbol of Negro violence. These symbols are always retained and sharpened.

Explanations added by the reporter to the description transmitted to him comprise a final form of sharpening. They represent a tendency to put "closure" upon a story which is felt to be otherwise incomplete. They illustrate the "effort after meaning" which customarily haunts the subject who finds himself in an unstructured situation. Such need for sharpening by explanation becomes especially strong when the story has been badly distorted and the report contains implausible and incompatible items. As an example, one subject who received a badly confused description of the subway scene FIGURE 1 inferred that there must have been "an accident." This explanation seemed plausible enough to successive listeners and so was not only accepted by them but sharpened in the telling.

In everyday rumors, sharpening through the introduction of specious explanations, is very apparent. Indeed, as we have said, one of the principal functions of a rumor is to explain personal tensions. To accept tales of army waste or special privilege among OPA officials

could "explain" food shortages and discomfort. Such stories, therefore, find wide credence.

Here, perhaps, is the place to take issue with the popular notion that rumors tend to expand like snowballs, become over-elaborate, and verbose. Actually, the course of rumor is toward brevity, whether in the laboratory or in everyday life. Such exaggeration as exists is nearly always a sharpening of some feature resident in the original stimulus-situation. The distortion caused by sharpening is, of course, enormous in extent; but we do not find that we need the category of "elaboration" to account for the changes we observe.

Assimilation

It is apparent that both leveling and sharpening are selective processes. But what is it that leads to the obliteration of some details and the pointing-up of others; and what accounts for all transpositions, importations, and other falsifications that mark the course of rumor? The answer is to be found in the process of *assimilation*, which has to do with the powerful attractive force exerted upon rumor by habits, interests, and sentiments existing in the listener's mind.

Assimilation to Principal Theme. It generally happens that items become sharpened or leveled to fit the leading motif of the story, and they become consistent with this motif in such a way as to make the resulting story more coherent, plausible, and well rounded. Thus, in Protocol A, the war theme is preserved and emphasized in all reports. In some experiments using the same picture, a chaplain is introduced, or people (in the plural) are reported as being killed; the ambulance becomes a Red Cross station; demolished buildings are multiplied in the telling; the extent of devastation is exaggerated. All these reports, false though they are, fit the principal theme—a battle incident. If the reported details were actually present in the picture, they would make a "better" *Gestalt*. Objects wholly extraneous to the theme are never introduced—no apple pies, no ballet dancers, no baseball players.

Besides importations, we find other falsifications in the interest of supporting the principal theme. The original picture shows that the Red Cross truck is loaded with explosives, but it is ordinarily reported as carrying medical supplies which is, of course, the way it "ought" to be.

The Negro in this same picture is nearly always described as a

soldier, although his clothes might indicate that he is a civilian partisan. It is a "better" configuration to have a soldier in action on the battlefield than to have a civilian among regular soldiers.

GOOD CONTINUATION. Other falsifications result from the attempt to complete incompleted pictures or to fill in gaps which exist in the stimulus field. The effort is again to make the resulting whole coherent, and meaningful. Thus, the sign, "Loew's Pa . . .," over a moving picture theater is invariably read and reproduced as "Loew's Palace" and Gene *Antry* becomes Gene *Autry*. "Lucky Rakes" are reported as "Lucky Strikes."

All these, and many instances like them, are examples of what has been called, in *Gestalt* terms, "closures." Falsifications of perception and memory they are, but they occur in the interests of bringing about a more coherent, consistent mental configuration. Every detail is assimilated to the principal theme, and "good continuation" is sought, in order to round out meaning where it is lacking or incomplete.

ASSIMILATION BY CONDENSATION. It sometimes seems as though memory tries to burden itself as little as possible. For instance, instead of remembering two items, it is more economical to fuse them into one Instead of a series of subway cards, each of which has its own identity, reports sometimes refer only to "a billboard," or perhaps to a "lot of advertising" (FIGURE 1). In another picture, it is more convenient to refer to "all kinds of fruit," rather than to enumerate all the different items on the vendor's cart. Again, the occupants of the car come to be described by some such summary phrase as" several people sitting and standing in the car." Their individuality is lost.

ASSIMILATION TO EXPECTATION. Just as details are changed or imported to bear out the simplified theme that the listener has in mind, so too many items take a form that supports the agent's habits of thought. Things are perceived and remembered the way they *usually* are. Thus, a drugstore in one stimulus-picture, is situated in the middle of a block; but, in the telling, it moves up to the corner of the two streets and becomes the familiar "corner drugstore." A Red Cross ambulance is said to carry medical supplies rather than explosives, because it "ought" to be carrying medical supplies. The kilometers on the signposts are changed into miles, since Americans are accustomed to having distances indicated in miles.

The most spectacular of all our assimilative distortions is the find-

ing that, in more than half of our experiments, a razor moves (in the telling) from a white man's hand to a Negro's hand (FIGURE 1). This result is a clear instance of assimilation to stereotyped expectancy. Black men are "supposed" to carry razors, white men not.

ASSIMILATION TO LINGUISTIC HABITS. Expectancy is often merely a matter of fitting perceived and remembered material to pre-existing verbal clichés. An odd example is found in the case of a clock tower on a chapel. In the telling, the chapel becomes a "chaplain" and the clock, having no place to go, lands on a fictitious mantelpiece.

> *Sixth Reproduction:* This is a picture of a battlefield. There is a chapel with a clock which says ten minutes to two. A sign down below gives the direction to Paris and Paris is 50 miles, and Cherbourg 21 miles away. People are being killed on the battlefield.
>
> *Seventh Reproduction:* This is a picture of a battlefield. There is a chaplain, and a clock on the mantelpiece says ten minutes to two. There is a sign, so many miles to Cherbourg.

The powerful effect that words have in arousing images in the listener and fixing for him the categories in which he must think of the event is, of course, a major step in the conventionalization of rumor. A "zoot-suit sharpie" arouses a much more compelling image (capable of assimilating all details to itself) than more objective words, such as "a colored man with pegged trousers, wide brimmed hat, etc." (FIGURE 1). Rumors are commonly told in terms of verbal stereotypes. Over and over again, they include prejudicial judgment, such as "draft dodger," "Japanese spy," "brass-hat," "dumb Swede," "long-haired professor," and the like.

MORE HIGHLY MOTIVATED ASSIMILATION

Although the conditions of our experiment do not give full play to emotional tendencies underlying gossip, rumor, and scandal, such tendencies are so insistent that they express themselves even under laboratory conditions.

ASSIMILATION TO INTEREST. It sometimes happens that a picture containing women's dresses, as a trifling detail in the original scene, becomes, in the telling, a story exclusively about dresses. This sharpening occurs when the rumor is told by groups of women, but never when told by men.

A picture involving police was employed with a group of police officers as subjects. In the resulting protocol, which follows, the entire reproduction centers around the police officer (with whom the subjects undoubtedly felt keen sympathy or "identification"). Furthermore, the nightstick, a symbol of his power, is greatly sharpened and becomes the main object of the controversy. The tale as a whole is protective of, and partial to the policeman.

Protocol B

Description from the screen: This is an excerpt from a motion picture that appeared in a national magazine. The scene is Detroit during the colored-white riot. There is a crowd around a police officer with a riot stick in his right hand and a Negro sitting on the ground, holding to his leg. On the right a boy is running away. On the left, facing the officer is a man who looks hostile but is afraid to go nearer because of the riot stick. The crowd comprises approximately 100 people.

First Reproduction: The picture on the screen is an excerpt from a motion picture taken at the time of the Detroit riot. In the picture, a police officer with a stick in his right hand is standing over a man on the ground. On the right, is a small boy; on the left, is a man who wants to interfere but is afraid of the policeman's stick.

Second Reproduction: This is an excerpt from a movie taken at the time of the Detroit riot. There is an officer with a stick in his hand and a man on the ground. There is a small boy and a man who wants to interfere but is afraid.

Third Reproduction: Picture was taken during the Detroit riot. There is a man in the picture, also a police officer. The man has a stick in his hand and wants to interfere, but does not for some reason. There is also a child.

Fourth Reproduction: This is a picture of the Detroit riot showing a policeman and a civilian. The policeman has a billy in his hand and the man wants to take it away from him.

Fifth Reproduction: A picture of the Detroit riot. There is a police officer with a club. Somebody wants to take it away from him.

Protocols based on the same picture, taken from a group of subjects who were not policemen, show how, in a different group, the focus of interest and direction of sympathy may be quite different. Only the police tell rumors that favor the police.

Assimilation to Prejudice. Hard as it is in an experimental situation to obtain distortions that arise from hatred, yet we have in our material a certain opportunity to trace the hostile complex of racial attitudes.

We have spoken of the picture which contained a white man holding a razor while arguing with a Negro. In over half of the experiments with this picture, the final report indicated that the Negro (instead of the white man) held the razor in his hand, and several times

he was reported as "brandishing it wildly" or as "threatening" the white man with it (FIGURE 1).

Whether this ominous distortion reflects hatred and fear of Negroes we cannot definitely say. In some cases, these deeper emotions may be the assimilative factor at work. And yet the distortion may occur even in subjects who have no anti-Negro bias. It is an unthinking cultural stereotype that the Negro is hot-tempered and addicted to the use of razors as weapons. The rumor, though mischievous, may reflect chiefly an assimilation of the story to verbal-clichés and conventional expectation. Distortion in this case may not mean assimilation to hostility. Much so-called prejudice is, of course, a mere matter of conforming to current folkways by accepting prevalent beliefs about an out-group.

Whether or not this razor-shift reflects deep hatred and fear on the part of white subjects, it is certain that the reports of our Negro subjects betray a motivated type of distortion. Because it was to their interest as members of the race to de-emphasize the racial caricature, Negro subjects almost invariably avoided mention of color. One of them hearing a rumor containing the phrase, "a Negro zoot-suiter," reported "There is a man wearing a zoot suit, *possibly* a Negro."

For one picture, a Negro reporter said that the colored man in the center of the picture "is being maltreated." Though this interpretation may be correct, it is likewise possible that he is a rioter about to be arrested by the police officer. White and Negro subjects are very likely to perceive, remember, and interpret this particular situation in quite opposite ways.

Thus, even under laboratory conditions, we find assimilation in terms of deep-lying emotional predispositions. Our rumors, like those of everyday life, tend to fit into, and support, the occupational interests, class or racial memberships, or personal prejudices of the reporter.

Children

Our findings showed a striking lack of interest among children in the racial identity of characters in the picture. A "Negro" was often reported simply as a "man." In the case of pictures disclosing racial identity, 85 per cent of our adult protocols mentioned this identity, while only 43 per cent of protocols taken from children did so.

Though we do not have extensive data from different age levels, it

appears certain that the younger the child, the less he is likely to report ethnic character. One is reminded of the case of Tommy, aged six, who asked his mother if he might bring his schoolmate Sam home to lunch next day. Knowing that Tommy was in a "mixed" school, his mother asked if Sam was a Negro. Tommy replied, "I didn't notice, but I'll look and tell you tomorrow."

Our experiment offers an opportunity to study the growing importance of ethnic identity in word of mouth stories told by children of successive ages.

Conclusion: The Embedding Process

Leveling, sharpening, and assimilation, are not independent mechanisms. They function simultaneously, and reflect a singular subjectifying process that results in the autism and falsification which are so characteristic of rumor. If we were to attempt to summarize what happens in a few words we might say:

Whenever a stimulus field is of potential importance to an individual, but at the same time unclear, or susceptible of divergent interpretations, a subjective structuring process is started. Although the process is complex (involving, as it does, leveling, sharpening, and assimilation), its essential nature can be characterized as an effort to reduce the stimulus to a simple and meaningful structure that has adaptive significance for the individual in terms of his own interests and experience. The process begins at the moment the ambiguous situation is perceived, but the effects are greatest if memory intervenes. The longer the time that elapses after the stimulus is perceived the greater the threefold change is likely to be. Also, the more people involved in a serial report, the greater the change is likely to be, until the rumor has reached an aphoristic brevity, and is repeated by rote.

Now, this three-pronged process turns out to be characteristic not only of rumor but of the individual memory function as well. It has been uncovered and described in the experiments on individual retention conducted by Wulf, Gibson, Allport,[6] and, in Bartlett's memory experiments carried out both on individuals and on groups.[7]

Up to now, however, there has been no agreement on precisely the terminology to use, nor upon the adequacy of the three functions we

[6] Conveniently summarized in **K. Koffka** Principles of Gestalt Psychology. Harcourt Brace & Co. New York. 1935.
[7] **F. C. Bartlett** Remembering. Cambridge University Press. 1932.

here describe. We believe that our conceptualization of the three-fold course of change and decay is sufficient to account, not only for our own experimental findings and for the experiments of others in this area, but also for the distortions that everyday rumors undergo.

For lack of a better designation, we speak of the three-fold change as the *embedding* process. What seems to occur in all our experiments and in all related studies is that each subject finds the outer stimulus-world far too hard to grasp and retain in its objective character. For his own personal uses, it must be recast to fit not only his span of comprehension and his span of retention, but, likewise, his own personal needs and interests. What was outer becomes inner; what was objective becomes subjective. In telling a rumor, the kernel of objective information that he received has become so embedded into his own dynamic mental life that the product is chiefly one of projection. Into the rumor, he projects the deficiencies of his retentive processes, as well as his own effort to engender meaning upon an ambiguous field, and the product reveals much of his own emotional needs, including his anxieties, hates, and wishes. When several rumor-agents have been involved in this embedding process, the net result of the serial reproduction reflects the lowest common denominator of cultural interest, of memory span, and of group sentiment and prejudice.

One may ask whether a rumor must always be false. We answer that, in virtually every case, the embedding process is so extensive that no credibility whatever should be ascribed to the product. If a report does turn out to be trustworthy, we usually find that secure standards of evidence have somehow been present to which successive agents could refer for purposes of validation. Perhaps the morning newspaper or the radio have held the rumor under control, but when such secure standards of verification are available, it is questionable whether we should speak of rumor at all.

There are, of course, border-line cases where we may not be able to say whether a given tidbit should or should not be called a rumor. But if we define rumor (and we herewith propose that we should), as *a proposition for belief of topical reference, without secure standards of evidence being present*—then it follows from the facts we have presented that rumor will suffer such serious distortion through the embedding process, that *it is never under any circumstances a valid guide for belief or conduct.*

SECTION OF ANTHROPOLOGY

November 26, 1945

Doctor Francis L. K. Hsu, Formerly Professor of Social Anthropology, National Yunnan University, Kunming, China; Lecturer of Anthropology, Columbia University, New York: *The Problem of Education in Contemporary China.*

Education, A Problem of Cultural Transition

In any society, the problem of education is one of transmitting a large or small part of its culture from one generation to another. The amount and variety transmitted are usually regarded as necessary for the younger generation to exist and function as fully as possible in the society.

There are two broad classes of problems involved. Where the society is not in a state of rapid change, the process of transmission is made comparatively easy. In such a configuration, family, school and the wider society more or less work together. The individual is likely to be able to pass from one stage to another without significant trouble; and the society is likely to be able to absorb products of the schools.

Where the society is in a state of rapid change, the process of transmission is much more complicated. Here, the problem involves not only cultural transition from the older to the younger generation; but also a change from old to new standards for all, as well as the ability of the society to assimilate the products under new standards. Here, the problem of education concerns not only rules of social behavior and methods of using tools, but also mores connected with the use of such tools.

Education in contemporary China is a problem which belongs to the latter category. It is a situation in which family, school and the society fail, at least temporarily, to conform to each other's standards. standards.

Education in Traditional China

In order to understand education in contemporary China, it is

necessary to grasp its basic features in traditional China. By traditional China is meant here China before 1842, when Chinese society was free from significant western influences and was more or less a self-contained whole. In that China, informal education had three objects. The first object was education for livelihood. This kind of education was mainly divided according to sex, one of the cardinal principles of the kinship structure. In this case, the male was taught to carry on one type of activities and the female, a different kind. Sons followed their fathers, while daughters took after their mothers. Sometimes, there was overlapping. Nevertheless, the two centers of activity were unmistakable.

The second object was education for social adequacy. This was again based upon two main principles of paramount importance in the kinship structure. The first principle was the supremacy of the authority of the parents, especially that of the father. The second was the suppression of the expression of sex. The latter called not only for the suppression of the expression of sex in the narrower sense, such as that which is related to lovemaking or sexual intercourse, but also for the suppression of any expression of affection between man and wife.

Under this education, the socially adequate person was he who was exceedingly filial to his parents, who maintained harmonious relations with his brothers and relatives, and who minimize, to the best of his ability, his feelings for members of his individual family circle, that is, his wife and unmarried children. He never tried to improve things in any channels, except those paternally sanctioned. The younger generation was taught after the pattern of the older.

The third object in the older education was ritual adequacy. Ritual adequacy was really supplementary to social adequacy. The world of spirits was a close copy of the world of humans. In that world, there were at least two main groups of spirits and a vast number of higher gods. In one group, were the dead members of the family, the dead relatives and dead friends. In a second group were the spirits of those who were not related friends. Spirits of family members, relatives and friends were always friends toward the particular family to which they belonged, as well as to their living friends and relatives. But the spirits who did not belong to this category were of uncertain disposition. They might be friendly if they were pleased, or they

might be harmful when they were antagonized, or they might be indifferent. The individual must either please them positively, or, by remaining negative, at least, not offend them. The higher gods upheld all the qualities encouraged in social adequacy and would punish significant culprits who sinned against the same qualities.

In traditional China, formal education was a part of informal education. By formal education, is meant institutionalized education, such as that given in schools and universities.

In such an education, a livelihood was to be made by studying the classics and working to pass a number of successive examinations. Successful candidates would enjoy opportunities of becoming members of the official hierarchy. In traditional China, there was no significant industry other than agriculture. Officialdom, which absorbed the more energetic elements of the society, was practically an industry under the circumstances.

In relation to social adequacy, formal education merely emphasized the rules upheld by informal education, but it went further.

The qualities encouraged in both types of education were similar, but a learned person was expected to show greater adherence to classical rules in matters of filial piety, sex behavior, etc.

Theoretically, Chinese scholars were not supposed to take too much notice of the spiritual world. Confucius was known for his attitude of indifference toward the world of spirits. Nevertheless, as most ritual matters were communal matters, and all scholars were taught to be harmonious with the communities in which they lived and participated, as members, this theoretical indifference was of no material consequence.

In traditional China, formal education was under the close supervision of the family. When a person had reached the top, through formal education, he would come back to the family, through the belief that an individual owed all to his ancestors.

Transitional China

By transitional China, is meant China since 1842, and especially after 1911, when she became a republic, and when all traditional institutions of education and examination were formally abolished. It was in the latter period, that adherence to Western influences became,

no longer, a crime to be punished. As a matter of fact, Western influences had definitely become more fashionable.

Informal education remained, however, the same as before, for the vast majority of the Chinese. As a result, formal education, as given in schools, and informal education, as given in homes and neighborhoods, were often conflicting propositions. For school education was greatly changed in scope, method, and results. The first thing to be mentioned was the abolition of the old imperial examination system. The second was the fact that the new schools taught new subjects and were no longer under the control of the family. More schools were established through government, missionary and commercial efforts. Also, schools became more expensive. The third important point was the fact that the literati as a group lost some of their old prestige. Formerly, a scholar who had passed the elementary examination was entitled to certain privileges in any community. Now, a primary school graduate, or even a middle school graduate, not in command of any post, does not enjoy nearly the same prestige, as formerly, in the eyes of the illiterate public. The fourth factor is the series of social conflicts that resulted. These conflicts are many and varied but they may be reduced to four general categories.

One category of social conflict appears in connection with sanitation and health. School children are taught not to spit on the floor, to take baths regularly, to have injections when there is a cholera epidemic, etc. When they go home, however, they find the barrier of age-old habits, deeply entrenched in the minds of their parents and senior relatives, practically insurmountable.

The second category of social conflict concerns sex. This conflict expresses itself in two ways. It involves the question of sex mores. Some traditional ideas stipulated that women were unclean, especially those in their monthly periods or after childbirth. Other traditional ideas prescribed that men and women should not meet freely and become friends. Some missionary schools have tried their best to meet the second of these traditional requirements by censoring letters of their students and expelling boys and girls who were found guilty of having communicated privately with each other. But, on the whole, the norm of behavior taught in the new schools departs further and further from that which has been upheld for centuries. The second difficulty concerns the choice of a life partner. There have been many tragedies in

which a man's educational level and that of his wife were vastly different. They were married by parental authority when they were young. Later on, the man went to school and college while his wife stayed behind. There are many instances in which the man, in such a situation, worked very hard and succeeded in raising the educational level of his ignorant wife, but there are other instances in which no happy solution could be found.

A third category of social conflict concerns family bonds. Parental authority is being broken more and more by a greater expression of exclusive intimacy between man and wife. Needless to say, many parents find this experience very bitter. Another point in this category is that, according to the traditional code, a man who is in a position of influence should help all his relatives by giving them jobs and other assistance, without seriously considering their suitability for the jobs, or the assistance which they require. Nowadays, many younger and idealistic people have a hard time in struggling against this age-old harness.

The fourth category of social conflict may be called unemployment among the educated. Unemployment among the *literati* has always been a fact in traditional China. Everybody knows about Peking and its hundreds of thousands of scholars waiting for official posts, long before 1842. But since the revolution of 1911, this unemployment has changed its character considerably, in the following ways:

Formerly, the aim of any and every scholar was clear. He had to study the classics, pass the examinations and then look for a post in the official hierarchy. There was also a general attitude of reverence among the public toward any and every scholar. There was no other place for members of the *literati* than in the official hierarchy. Those who were unsuccessful in attaining official posts could always have something to fall back on at home in their own community, where they would be supported in one way or another.

Now, the aim among the educated has become less clear. There are, no longer, imperial examinations. There is also the general, but vague idea that scholars should not look toward official hierarchy as their only salvation. There are all kinds of ideas concerning social and economic improvement of the country as a whole. As the result of contact with the West, a wide variety of acute needs has arisen. Some of these have existed before but never have been so clearly felt and defined,

until now. But the new members of the *literati* are often not equal to most of the tasks. Some of the difficulties involved are the following: (1) The new techniques of social and economic improvement, which are taught in the school, cannot be applied very easily to a vast country with a long and ancient tradition. Among Chinese literati, there has never been any tradition or experience in organization for active purposes. Non-official organizations, even on a vast scale, have always existed in the country, but they were organized along established lines for purposes of maintaining the *status quo*. Few of them were organized for purposes of actively promoting ends which were not defined by tradition and which, in many ways, would have to contend with the huge, opposing forces of custom and inertia. Yet, the application of most of the newly acquired techniques in such fields as agriculture, cooperatives, and industries requires no less than modern types of organization.

The second difficulty is the matter of class distinction. From a vertical point of view, neither traditional China nor contemporary China has any class structure. That is to say, an individual who is able to rise above his station of birth by ability or other means is not handicapped because of his lower origin. On the other hand, at any given point of time, there was and is a clear class structure. In this, the most fundamental division has always been between manual workers, on the one hand, and literary workers, on the other. Many educated persons in the new situation are, therefore, finding themselves unable to take advantage of some of the newer opportunities, because they involve a certain loss of the *literati* status.

The third difficulty involved is the vast difference in standards of living between the educated class, on the one hand, and the majority of the population, on the other. Before the present Sino-Japanese war, the average salary of a university graduate, if he secured a job, would be about eight to ten times the salary of a waiter in a restaurant or of a shop assistant. The implication of this is perfectly clear. The vast majority of the industrial and commercial establishments had needs but were unable to offer rewards which would be equal to the standard of living of the educated.

The fourth difficulty is closely related to the third and is, of course, a most fundamental one. That is the lack of industrial development in China, as a whole. We may say that China is overpopulated, or we

may say that she is suffering from under-production. These are two ways of saying the same thing. Without fairly extensive industrial development, it was impossible to absorb the considerable number of men and women turned out by the new institutions of education. That was why needs in the vast majority of the traditional workshops and salesrooms and unemployment among the educated existed at the same time.

Without considering these fundamental points, some enthusiastic reformers have started the so-called college-student-go-back-to-village movement. These people and others exhorted products of modern education to go back to their rural districts with the intention of improving the life and conditions of millions of farmers while, at the same time, they would have to live, more or less, under the same conditions that they were trying to improve. This movement has two drawbacks: The first is that, without some sort of religious or other type of fanaticism, it has never been possible, throughout the known years of history of mankind, to get people, who are used to a higher level of comfort and have been taught the necessity of such comfort, voluntarily to give these up in favor of a life which is literally a fraction of their customary standard. The second drawback is more fundamental. These reformers have failed to recognize that reformation of rural conditions must be sustained by improvement of urban production, for the disastrous conditions of many Chinese villages are the result of forces beyond the control of the local communities and their inhabitants.

Prospect

There is no single answer to the problem of education in contemporary China. It is dependent upon a number of factors. There are two ultimate goals. First, universal literacy, or at least, literacy for the majority. Literacy, as such, is no guarantee for the attainment of democracy. But without a high degree of literacy, democracy on a large scale is impossible. With a higher degree of literacy will come a higher degree of separation of social considerations from technical matters. The sharp distinction, in terms of prestige and salaries, between manual and brain workers will be reduced. Technical requirements among the educated will be diversified. Literacy, as such, would no longer be a distinction by itself, because everybody will be literate or nearly so.

Secondly, a high degree of cooperation and coordination between

the three angles of the problem: home, school and society. A number of writers on modern Chinese education have emphasized the necessity of developing a kind of education which will be in keeping with the social conditions. The complaint is against many things which have been pointed out in the body of this paper, such as, for example, the fact that many graduates of schools and colleges who could not find employment or otherwise be absorbed by the society. There is nothing wrong with such complaints. What many writers have failed to realize is the fact that during the period of active social transformation, a good deal of dislocation in the home, school and the wider society is natural. In this configuration, the home usually represents the most conservative force. The school represents the more radical force, which has as its aim systematic introduction of new thoughts, behavior and technique into the society. Whereas, the wider society usually presents an uneven picture with some sections of it more conservative than others. It is essential to realize that unless we want the entire society to go back completely to its traditional form, we must emphasize the importance of the schools as the most important agency in this social transformation, and that it is the goal not to bend the institutionalized education to suit the conservative and backward conditions of the society, but to improve the latter and raise it gradually to the standard of the former.

Therefore, it will be well for thinkers on Chinese education to remember that the problem of education is first and foremost bound up with her economic developments. Industrialization, on a wide scale, is a legitimate aim, as ultimately essential to the solution of the problem of education in contemporary China. Industrialization will provide the resources for universal education. It will provide the contacts and opportunities among the population which will foster a desire for higher standards of living and this is an incentive to acquire more and better education. Until the people actively wish education on their own initiative, education on a large scale will never be realized. On the other hand, unless, through industrialization, the economic and other advantages are brought home to the parents and relatives, the people are not likely to want this education on their own. In other words, the home culture and the informal education it provides for the individual must be sufficiently in line with the aims and methods of

the school or formal education. And the success of both are again closely bound up with the wider economic development.

In this paper, I have merely analyzed and outlined some of the complex factors involved in the problem of education in contemporary China. The one significant factor I have not touched upon is the political framework which obviously will condition the whole configuration. The end result of a political framework which is molded upon giving the people greater freedom, equality of opportunities, and an increasing share in their own government will be diametrically opposed to that of a political framework which is determined to deprive the people of their freedom and equality of opportunities and any share in their own government.

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Elected November 29, 1945

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SECTION OF GEOLOGY AND MINERALOGY

DECEMBER 3, 1945

DOCTOR RALPH W. IMLAY, United States Geological Survey, Washington, D. C.: *Jurassic and Lower Cretaceous History of the Gulf Region.* (This lecture was illustrated by lantern slides.)

Lower and Middle Jurassic rocks in the Gulf region are known only in southern Mexico and northern Central America. If present in the southern United States, they have not yet been penetrated by drilling. If present in the Antilles, they must be included in the metamorphosed beds forming the basement rocks and have not been identified by fossils. Orogenic movements, near the end of the Triassic, produced geosynclinical conditions in southern Mexico and northern Central America, north of a rising land mass occupying the site of Honduras and southernmost Guatemala, Chiapas, and Oaxaca. Marine waters entered the region of Veracruz, early in Lower Jurassic time, and spread west and southwest as a rather narrow embayment that reached the region of northeastern Guerrero, by late Lower Jurassic time. Marine invasion became more widespread during the Middle Jurassic, as the sediments of that age in Oaxaca and Guerrero change from dominantly carbonaceous or coarse littoral, in their lower part, to sublittoral and normal marine, in their upper part. During the Lower and Middle Jurassic, the southern part of the geosyncline received from 2,000 to 3,000 feet of continental deposits, consisting mainly of vari-

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colored shale, sandstone, conglomerate and coal. Much of the shale is carbonaceous and contains numerous plant remains. Some marl and limestone are interbedded with this continental facies, particularly in its upper part. Farther north, in the Huesteca region of Vera Cruz, eastern Hidalgo and northern Puebla, the Lower Jurassic is represented by more than 1,300 feet of dark, marine shale containing numerous ammonites. Well-preserved plants, occurring at the top of the shale and also associated with ammonites in the lower part of the shale, furnish a correlation with the lower part of the continental facies. The littoral and sublittoral character of most of the Lower and Middle Jurassic sediments and the excellent preservation of some of the plant fossils indicate that marine waters did not occupy much area, but probably occurred as bays and lagoons. The climate must have been hot and humid, at least seasonally, as indicated by the richness of the flora, the presence of considerable coal, and the dark color of much of the shale. The red, brown and yellow colors of some of the sandstone and conglomerate do not constitute evidence of aridity, as these sediments are interbedded with dark, carbonaceous and coaly sediments.

During Upper Jurassic time, marine waters spread widely over both southern and northern Mexico, the southern United States, and Cuba. Thicknesses of sediments deposited are in thousands of feet, ranging from 3,000 to 7,000 feet, in the southern United States; from 2,600 to 4,800 feet, in northern Mexico; at least 5,000 feet, in southern Mexico, and at least 6,000 feet, in Cuba. The remarkable uniformity in composition of the Upper Jurassic throughout most of the Gulf region is ascribed to the peneplained condition and the high aridity of the continent, near the beginning of Upper Jurassic time.

During the Callovian stage at the beginning of the Upper Jurassic, the marine waters, according to present knowledge, were restricted to southern Mexico, but were much more widespread than previously in the Jurassic. The deposits consist of shale, marl, limestone, and some sandstone that are mainly dark-colored and bituminous and are locally over 2,000 feet thick. These beds grade downward into the Middle Jurassic marine and continental beds, but their relationship to the overlying Jurassic limestone is not known. They have furnished many ammonites, which show that both the lower and upper Callovian are represented. Their dark color and the considerable quantity of shale suggest that the surrounding land masses had a fairly humid climate.

The Divesian stage is probably represented in northern Central America, southern Mexico and the southern United States by a salt facies that grades shoreward into a red bed facies. A thick red bed facies, at least partly continental in origin, was formed throughout northern Mexico. The distribution of salt in southern Mexico is shown mainly by salt domes and salt springs. Its distribution in the United States is shown by bedded salt deposits around the margins of the basins and by penetration salt domes in the centers of the basins. According to R. H. Palmer, several domelike structures in northern Cuba appear to be salt domes. The salt and red beds are represented in the southern United States by the Eagle Mills formation, that ranges in thickness from 960 to over 1,780 feet and rests unconformably on late Paleozoic rocks. Its Divesian age is shown by gradation of the salt facies into the overlying Smackover limestone and by the presence of upper Argovian ammonites, only 300 feet above the salt. Correlation with the salt and red beds of southern Mexico is indicated by their similar stratigraphic position and by the considerations that both areas of salt deposition were of immense size, were undoubtedly connected with the Gulf of Mexico and were influenced by similar climates. The possibility that the salt in southern Mexico is older than Divesian is very improbable, considering that the older Jurassic beds are well represented by marine fauna that could not have lived in a highly saline environment and by rich flora that, according to Wieland, indicate a tropical climate of the Monsoon forest type. That the climate of Divesian time was particularly favorable for deposition of rock salt is shown by the presence of salt in northern Utah and southeastern Idaho in the medial part of the San Rafael group, below a marine unit of Argovian age and above a marine unit of Callovian age. It seems improbable that the salt masses of Mexico and the southern United States were deposited in separate relic seas, because that would mean that there were two barriers, each about 800 miles long, existing simultaneously and having suitable conditions throughout which might permit replenishment of the waters of the salt basin, until about 1,000 feet of salt were deposited. Rather, the thickness and extent of the salt masses of the Gulf region suggest that the entire Gulf of Mexico was a salt depositing basin, completely enclosed, except for a relatively narrow, shallow strait connecting with the Atlantic Ocean, perhaps in the region between Hispaniola and the Bahamas.

The climate must have been extremely arid for several million years, probably during most of the Divesian stage.

The Argovian stage is represented, in many parts of Mexico and the southern United States, by dark-colored limestone, called the Zuloaga limestone in Mexico and the limestone facies of the Smackover formation in the United States. The Smackover limestone differs from the Zuloaga limestone by containing many oolites in its upper part. Locally, the limestone is interbedded with much sandstone, conglomerate, and some shale which reflects local uplifts. One such area is in southern Coahuila and eastern Durango, where the sediments are known as La Gloria formation. Similar coarse sediments occur in northern Louisiana and are considered a facies of the smackover formation. Thicknesses of rocks of Argovian age range from a few hundred to more than 2,000 feet. Sites of salt deposition during Divesian time gradually became sites of lime deposition during Argovian time, but elsewhere, as in northern Mexico, the Argovian sea transgressed across red beds and older rocks. The Argovian stage in Cuba is represented, according to R. H. Palmer, by about 400 feet of shaly limestone called the Jagua formation. The remarkably uniform appearance of the Argovian sediments throughout most of the Mexican and Gulf Coast geosynclines indicates a uniform, arid climate, few rivers emptying into the sea, and few highland areas. Aridity, plus the peneplained condition of the interior of the continent, account for the scarcity of rivers and for the deposition of calcareous sediments near shore. Correlation of the Smackover formation with the Zuloaga limestone of Mexico and the Jagua formation of Cuba is based on the presence of the ammonites, *Dichotomosphinctes* and *Discosphinctes,* on the ranges of some gastropod genera, and on the occurrence, in the overlying beds, of lower and middle Kimmeridgian ammonites.

Early in the lower Kimmeridgian, marine waters retreated basinward from 50 to 100 miles, giving rise to extensive lagoons, in which accumulated thick masses of anhydrite and red beds. These are known as the Buckner formation, in the southern States, as the Olvido formation, in the Sierra Madre Oriental; and are included in the basal part of the Malone formation in western Texas. The deposits range in thickness from 50 to 1,000 feet and are succeeded basinward by normal marine, dark-colored sediments, which, in Louisiana, have furnished lower Kimmeridgian ammonites of the *genera Ataxioceras* and *Idoceras.*

At the end of the deposition of the anhydrites and red beds of the early Kimmeridgian, occurred the most intense orogeny in North America since the Paleozoic. Orogeny was expressed by block faulting (Palisade disturbance), in the Atlantic Coast region; by the Nevadian orogeny of the Pacific Coast region; by the development of highlands, in the areas of the Ouachita Mountains, the Central Mineral region, the Diablo Plateau, the Coahuila Platform, the eastern margin of the Sierra Madre Oriental and, probably, in many other places. A gentle elevation of the interior of the continent drained the Sundance sea from the area of the United States and caused a temporary retreat of the Gulf waters. All available evidence indicates that the orogeny began early in the Kimmeridgian and was complete by the upper Portlandian. Evidence for the orogeny in the eastern United States is furnished mainly by the thick mass of coarse conglomerates at the base of the Cotton Valley formation. In southwestern Alabama, conglomerates range throughout the lower 1,660 feet of the formation and, according to core and drilling records, contain some boulders more than a foot in diameter.

Late in the lower Kimmeridgian, marine waters transgressed widely in the northern parts of the Gulf of Mexico and the Mexican sea, but Cuba apparently remained a land area until the upper Portlandian. The late Upper Jurassic deposits of the Gulf region range in thickness from a few hundred to a few thousand feet. The offshore deposits consist mainly of bituminous shale and limestone, which, in the southern United States, are known as the normal marine facies of the Cotton Valley group; in Mexico, as La Caja formation, and, in Cuba, as the Quemado formation, and the Viñales limestone. Nearshore deposits contain much conglomerate and sandstone, in addition to shale. They are known, in northern Mexico, as La Casita formation, in western Texas, as the Malone formation, and, in the southern States, are included in the Schuler formation of the Cotton Valley group. The northern part of the Mexican sea was bordered by lagoons, in which some gypsiferous and coaly deposits accumulated. The late Upper Jurassic age of the Cotton Valley formation is shown by the presence in its lower part of many Kimmeridgian fossils including *Idoceras* group of *I. durangense* (Burckhardt) and *Glochiceras fialar*. The climate of the late Upper Jurassic was moister than during the early Upper Jurassic, as shown by the presence of coal beds and carbonace-

ous to bituminous shales. Marine waters retreated slightly from the northern part of the Gulf region at the end of the Jurassic, but uplift was much less than during the early Kimmeridgian, as shown by the thickness and extent of the gravels at the base of the Lower Cretaceous. Erosion of the highlands, formed during the mid-Upper Jurassic orogeny, eventually produced the Fall zone peneplain.

The Lower Cretaceous of the Greater Antilles is little known. Late Lower Cretaceous rocks, similar to the Glen Rose limestone of Texas, have been found in Cuba. Some geologists consider that the Viñales limestone (*Aptychus* limestone) may be partly of early Lower Cretaceous age, although the only ammonites that it has furnished are of late Jurassic age. It would not be surprising to find a considerable sequence of Lower Cretaceous rocks in Cuba or in the other large islands of the Greater Antilles.

The Lower Cretaceous of Central America and Mexico is fairly well known in broad outlines. During the Neocomian, the Mexican sea was confined to the central part of the Mexican geosyncline, was indented by peninsulas in Coahuila and Oaxaca, overlapped islands in eastern Mexico and probably extended westward through Colima and Jalisco and southeastward into northern Central America. Its western margin in the area of Chihuahua, Durango and Zacatecas is still not known. Coarse clastic sediments were formed nearshore mainly during the Berriasian, lower Valanginian and upper Hauterivian. They show considerable variation in character and thickness, from place to place, probably depending on the positions of rivers or of local uplifts. Thin-bedded limestone and marl are interbedded with these coarse sediments and are the characteristic offshore rocks from Berriasian to Hauterivian. The Barremian is represented offshore mainly by thick- to medium-bedded limestone, but nearshore contains some intercalations of shale and sandstone and, locally, gypsum and rock salt.

During the lower Aptian, the Mexican sea was apparently nearly as restricted as during the Neocomian, but, during the upper Aptian and Albian, it spread widely and possibly connected with the Pacific Ocean in both northern and southern Mexico. Throughout central and northern Mexico, a major change in sedimentation occurred in the lower Cenomanian, apparently coinciding with the unconformity between the Comanche and Gulf series in the southern United States.

The lower Aptian of the Mexican geosyncline consists mainly of thick- to medium-bedded limestone, but nearshore contains some shaly

beds, arkosic sandstones and, locally, gypsum and salt. The upper Aptian and basal Albian are represented widely by shale and thin-bedded limestone, but in southeastern Puebla and in Sonora contain much coarse material. The middle Albian and the upper part of the lower Albian are represented by a number of facies. Thin-bedded limestone, interbedded with black chart, was deposited throughout the central part of the Mexican geosyncline. This facies grades marginally and rather abruptly into rudistid- or Orbitolina-bearing limestone which is generally much thicker and was deposited over areas that were landmasses in Jurassic or Neocomian times. This facies, in turn, changes abruptly into a dense, thick-bedded, nonrudistid-bearing limestone which occurs in the Sierra Madre Oriental and the coastal plain north of southern Tamaulipas. In western Coahuila, gypsiferous beds were formed over the site of the Coahuila Peninsula. In east-central Sonora, thousands of feet of limestone, shale, agglomerate and lava were deposited. The upper Albian is represented mainly by wavy-bedded, thin-bedded limestone and many lenses of black chert.

The Lower Cretaceous, penetrated by drilling in southern Florida, consists of a considerable thickness of limestone and anhydrite, of Albian age. In contrast, the lower Cretaceous in southwestern Alabama consists of nearly 4,900 feet of sandstone, reddish shale and some gravel, but contains minor amounts of pink and gray, nodular limestone, toward the top. Similar sections of sandstone and red shale have been found in southern Georgia, northern Florida and in Mississippi. In the western and southern parts of Mississippi, the upper part of the Lower Cretaceous includes some limestone and anhydrite, resembling the Lower Cretaceous farther west.

In the Arkansas-Louisiana-East Texas area, the advancing sea of early Cretaceous time encountered large amounts of gravel derived from the Ouachita Mountain area and spread it widely in southern Arkansas and eastern Texas. The gravel was succeeded upward by large amounts of red to white sand and generally reddish silts and clays which are locally over 2,000 feet thick. These sediments have been named the Hosston formation and are considered to be mainly of Neocomian age. They were succeeded transitionally and transgressively, during Aptian and lower Albian times, by about 350 to 1,200 feet of normal marine sediments, comprising the subsurface Sligo formation, Pine Island shale, James limestone, and Rodessa formation

and consisting of interlensing oolites, coquinas, marls, silt, clay and sand. Toward the end of Rodessa time, several widespread anhydrite layers were formed. The succeeding Ferry Lake anhydrite is unusually interesting. It ranges from 10 to 500 feet in thickness, extends about 800 miles from western Mississippi to eastern Texas, occupies a bed about 150 miles wide, and grades southward into normal marine shale and limestone. It was overlain conformably by as much as 700 feet of clay, silt, lime mud and sand, called the Mooringsport formation. Then followed deposition of the Paluxy sands and variegated clays along the northern margin of the Gulf region, from East Texas eastward. The overlying, relatively thin Fredericksburg limestones and shales of late middle Albian age have been identified as far east as western Mississippi. The Washita limestone of upper Albian age has been definitely identified only as far east as extreme southwestern Arkansas, but the occurrence of *Alectryonia* cf. *A. quadriplicata* (Shumard) in well cores from Tensas Parish, northeastern Louisiana, suggests that the upper Albian limestones may once have extended that far east. Their apparent absence, east of the Mississippi River, may be due to non-deposition, or to change of facies, or to erosion.

Lower Cretaceous sedimentation in South Texas was influenced by the presence of the Central Mineral region which furnished much less sand and clay to the bordering sea than did the highlands in the Ouachita Mountain areas. In comparison with the Arkansas-Louisiana-East Texas area, the Hosston formation is finer and more calcareous; the Sligo formation is thicker, more calcareous, and contains dolomite beds; the upper Aptian beds are similar in the two areas; the thick Glen Rose limestone lacks a persistent unit of anhydrite; the middle Albian sediments are considerably thicker, and the upper Albian sediments are in general thinner, except in the central part of the Rio Grande embayment. At the end of Comanche time, probably during the lower Cenomanian, the Arkansas-Louisiana-East Texas area was subjected to regional uplift, accompanied by extensive normal faulting which displaced the older rocks from 400 to 1,200 feet. Subsequently, the area underwent extensive erosion before deposition of the Gulf series. This uplift apparently had little effect in South Texas, and its influence east of the Mississippi River is unknown.

SECTION OF BIOLOGY

December 10, 1945

Doctor James B. Hamilton, Department of Anatomy, Long Island College of Medicine, Brooklyn, New York: *The Relationship Between Common Baldness and Male Sex Hormones.*

Common baldness (alopecia) is a sequela of sexual maturation and is, in most instances, induced by stimulation from male hormone substances. In keeping with this physiological relationship, the incidence of the disease is much higher in males than in females, and extensive forms of the disease are restricted almost entirely to males. Most women with pronounced forms of the disease are those with virilism.

Apparently, age is a factor in the rapidity with which areas of denudation extend. In eunuchs, susceptibility to such loss of hair can increase with age, but baldness does not occur because of inadequacy of testicular secretions. This increased susceptibility accumulates, unspent, like money in the bank, and later treatment of the eunuchs with male hormones (androgens) results in a rapid loss of hair. Available evidence does not bear crucially on the question of whether or not there is, with increasing age, a lowering of the amount of androgenic stimulation required to produce baldness.

The third factor known to be involved is inheritance. No amount of androgenic stimulation produces baldness in persons who lack an inherited tendency to this disease. The realization of this tendency depends, however, upon androgenic stimulation, since, whatever the inheritance, baldness does not ensue without androgenic stimulation.

Local areas of the skin play a dominant role in the atrophic changes that result in baldness. Application of androgens directly to a local area results in piliary changes limited to that region. At the present stage of our information, it must be assumed that androgens are only one of what might be a family of agents (although they are the usual one) which may be capable of inducing atrophic changes in specific areas. The degree of atrophy is controlled by the local areas of skin and does not result from external changes in vascularity or com-

pression of the area by tight bands. Rather, baldness will occur even in skin transplanted from its usual site.

Baldness is one of the so-called degenerative diseases which remain as extremely important medical problems and have not benefited from the achievements of chemotherapeutic control of infectious diseases. It is a condition, the progress of which can be studied. The nature and therapeutic control of the disease can be investigated carefully. Findings from studies of this disease may be presumed to be applicable to a number of the so-called degenerative diseases, like prostatic cancer, which are also prone to occur in the male sex.

SECTIONS OF BIOLOGY AND PHYSICS AND CHEMISTRY

DECEMBER 7 AND 8, 1945

Conference on *"Proteins and Protein Hydrolysates in Nutrition."*

The Sections of Biology, and Physics and Chemistry held a Conference on '"Proteins and Protein Hydrolysates in Nutrition." Doctor James B. Allison, Rutgers University, New Brunswick, New Jersey, was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Introductory Remarks, by James B. Allison.

"Amino Acid Deficiencies in Man," L. Emmett Holt, Jr., New York University, New York, N. Y.

"The Determination of the Nitrogen Balance Index in Normal and Hypoproteinemic Dogs," James B. Allison, John A. Anderson and Robert D. Seeley, Rutgers University, New Brunswick, New Jersey.

"The Evaluation of Protein Quality in the Normal Animal," Richard H. Barnes and David K. Bosshardt, Department of Biochemical Research, Sharp & Dohme, Inc., Glenolden, Pennsylvania.

"Electrophoretic Studies on Plasma Protein Depletion and Regeneration," Bacon F. Chow, Division of Protein Chemistry, The Squibb Institute for Medical Research, New Brunswick, New Jersey.

"Blood Protein Regeneration and Interrelation," G. H. Whipple, F. S. Robsheit-Robbins and L. L. Miller, The University of Rochester, School of Medicine and Dentistry, Rochester, New York.

"Nitrogen Metabolism in Acute and Chronic Disease," John P. Peters, Yale University, School of Medicine, New Haven, Connecticut.

"Clinical Observations Following the Intravenous Injection of a Protein Hydrolysate in Surgical Patients," Robert Elman, Washington University, School of Medicine, St. Louis, Missouri.

"Clinical Experience with Oral Use of Protein Hydrolysates," F. Co Tui, New York University, College of Medicine, New York, N. Y.

REPORT OF THE ANNUAL MEETING

December 19, 1945

The 128th Annual Meeting of the Academy, for the election of Officers, Fellows and Honorary Members, the presentation of reports and the transaction of other business was held at the Hotel Astor on the evening of Wednesday, December 19.

The Corresponding Secretary reported that there are now 42 Honorary Members upon the rolls of the Academy. The deaths of 6 Honorary Life Members were reported during the past year.

The Recording Secretary reported that, during the last year, the Academy held 27 Regular Sectional Meetings, in addition to its Annual Meeting, at which 27 stated papers of high scientific caliber were presented.

Ten informal receptions were held under the auspices of the various Sections of the Academy.

Seven two-day conferences on special subjects of research were held during the year. These meetings were attended by outstanding investigators in the fields represented. The titles of the conferences are as follows: Joint Meetings, Section of Biology and Section of Physics and Chemistry, "Blood Grouping," "Proteins and Protein Hydrolysates in Nutrition"; Section of Biology, "Experimental Hypertension," "Lymph"; Section of Physics and Chemistry, "Surface Active Agents," "Amino Acid Analysis of Proteins"; Section of Psychology, "Non-Projective Personality Tests."

Three Honorary Life Members, 2 Life Members, 7 Sustaining Members, 219 Active Members, 62 Associate Members and 14 Student Members were added to the rolls, of which 296 are now in good standing and 11 await qualification through payment of dues. Thus, a total of 307 new members was added during the year. Four Sustaining Members were transferred to Life Membership, having paid dues for twenty-five years. Twenty transfers and 7 reinstatements to membership were also effected between the various classes of membership, with the approval of the Council.

The Academy has lost by death 6 Honorary Life Members, 4 Life Members, 5 Sustaining Members, 11 Active Members and 2 Associate

Members. Thirty-one resignations were accepted, 31 more were dropped for non-payment of dues and 2 for not having qualified for membership.

The record now stands with a net gain of 221 members. There are at present on the rolls of the Academy 2,298 members, of whom 339 are Fellows. These include 1 Patron, 94 Life Members, 119 Sustaining Members, 1,353 Active Members, 651 Associate Members, 38 Student Members and 42 Honorary Life Members.

The Editor reported that, during the fiscal year of 1945, the Academy published a total of 584 pages. Of this amount, 346 pages were published in the Annals and 238 pages in the Transactions. This was less than the record of the previous year, due to the delays in printing occasioned by war priorities. In addition to the Articles published during the year, Articles 6, 7 and 8 of Volume 46, and Article 1 of Volume 47 are in press and will be issued shortly. Volume 46, Article 9 and Volume 47, Article 2 are in the Editor's hands and are being prepared for the printer.

The detailed list of the publications which have appeared this year is as follows:

ANNALS

Volume 46, Article 1, "Animal Colony Maintenance," (8 papers), by Edmond J. Farris, F. G. Carnochan, C. N. W. Cumming, Sidney Farber, Carl G. Hartman, Frederick B. Hutt, J. K. Loosli, Clarence A. Mills, Herbert L. Ratcliffe. Pages 1–126. Published June 15, 1945.

Volume 46, Article 2, "A Hitherto Undemonstrated Zoogleal Form of *Mycobacterium tuberculosis*," by Eleanor Alexander-Jackson. Pages 127–152. Published June 29, 1945. Awarded an A. Cressy Morrison Prize, 1944.

Volume 46, Article 3, "The Effect of Activity on the Latent Period of Muscular Contraction," by Alexander Sandow. Pages 153–184. Published June 30, 1945. Awarded an A. Cressy Morrison Prize, 1944.

Volume 46, Article 4, "Respiration and Germination Studies of Seeds in Moist Storage," by Lela V. Barton. Pages 185–208. Published August 18, 1945. Awarded Honorable Mention, A. Cressy Morrison Prize Competition, 1944.

Volume 46, Article 5, "The Diffusion of Electrolytes and Macromolecules in Solution" (6 papers), by L. G. Longsworth, Charles O. Beckmann, Margaret M. Bender, Edward M. Bevilacqua, Ellen B. Bevilacqua, Douglas M. French, A. R. Gordon, Herbert S. Harned, Lars Onsager, Jerome L. Rosenberg, and J. W. Williams. Pages 209–346. Published November 30, 1945.

TRANSACTIONS

Series II, Volume 7, Nos. 1–8, consisting of 238 pages, was completed, printed and distributed each month from November, 1944 to June, 1945, inclusive.

The Librarian reported that, during the year 1945, the Academy

distributed 26,893 separate publications and 3,624 complete volumes of the various published series, as follows:

Separate Numbers—*Annals*, 18,548: *Scientific Survey of Porto Rico and the Virgin Islands*, 997; *Transactions*, Series II, 7,343; *Memoirs*, 5.

Volumes—*Annals of the Lyceum*, 184; *Special Publications*, Volumes I and II, 257; *Transactions*, Series I, 19, and Series II, 3,164.

The Library of the Academy has received, from exchange institutions, 1,535 separate publications. With the cessation of hostilities and restoration of communications, shipment of the Annals and Transactions has been made to our exchange institutions, thus bringing many of them up to date, for the first time since September, 1939.

The Treasurer reported that the Academy, while pursuing its established program of progressive activity in the field of science, has also exhibited a corresponding financial development, continuing the progress of recent years.

During the past fiscal year, the total income received from all sources amounted to $39,380.83. This is an increase of $12,754.30 above that of 1944.

Through the hearty cooperation of its membership, the Academy added 307 new names and 7 reinstatements to its rolls, which represents a gain of $724.20 in our revenue from membership dues, making a total of $9,469.50 from this source.

Receipts from sales of publications and library exchanges amounted to $7,215.33.

The income from investments, amounting to $4,333.57, represents a yield of 4.79 per cent, a gain of $637.44 above that of last year.

Through the generosity of Mr. William Otis Sweet, a Sustaining Member, the sum of $2,000 has been added to his previous contribution, which established the William Otis Sweet Fund, last year. The principal of this fund now totals $3,500.*

A grant of $1,000 has been received from the Viking Fund to aid in defraying the cost of the paper by Doctor Earl W. Count, entitled "Brain and Body Weight in Man," which will appear shortly in the Annals.

Additional contributions, totaling $4,500, have been received from

* Announcement also was made at the Annual Meeting that Mr. Sweet had contributed the sum of $7,500 in addition to the above, thus bringing the total amount of the fund to $11,000.

the University of Puerto Rico and the Department of Agriculture and Commerce of Puerto Rico, toward the expense of publishing the Scientific Survey of Porto Rico and the Virgin Islands.

The increased income of the Academy, during the past year, has written off the debit balance of $1,905.30, shown in the Treasurer's report for the year ending November 30, 1944, and has produced a surplus amounting to $2,084.63, as of November 30, 1945.

Through the far-seeing advice of the Finance Committee, the capital funds of the Academy have shown a gain of $6,386.97. This is the margin above the liability to the endowment funds of the Academy.

The book value of the Academy's investments and cash on hand, as of November 30, 1945, now amounts to $96,958.62, as shown on the balance sheet.

The books of the Treasurer were duly checked and balanced at the end of the fiscal year, November 30, 1945, and have been audited by the Finance Committee, as provided by the Constitution and By-Laws.

The property of the Academy was verified and the Treasurer's report examined and attested by the Finance Committee, as of December 15, 1945.

The A. Cressy Morrison Prizes of $200 each for the two most acceptable papers in any field of natural science, within the scope of the Academy and its Affiliated Societies, were awarded to the following papers† entitled:

"Polyelectrons," by John Archibald Wheeler, Princeton University, Princeton, New Jersey.

"Limitations of Optical Image Formation," by Max J. Herzberger, Eastman Kodak Research Laboratories, Rochester, New York.

The Committee of Judges awarded Honorable Mention to the following paper, because of its general excellence:

"The Golgi Apparatus," by Leonard G. Worley, Department of Biology, Brooklyn College, Brooklyn, New York.

The Prize Committee extends its congratulations to the authors of these papers, on behalf of the Academy.

By authority of the Council of the Academy, the Committee takes pleasure in announcing that Mr. Morrison has renewed his offer of

† Abstracts of these papers are included in this issue of the Transactions.

two prizes of $200 each to be competed for during the year, 1946, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society. These prizes are to be awarded in December, 1946. The terms of the competition will be published in the next issue of the Transactions.

The Committee also announces, on behalf of Mr. Morrison, that he has offered an Astronomical Prize of $500, in renewal of those awarded in recent years, for the paper, adjudged by the Council of the Academy to be the most meritorious contribution on the subject of solar and stellar energy. In connection with this offer, the following statement has been prepared:

Understanding of the source of solar and stellar energy begins with Helmholtz's contraction theory (1854). As the primordial star contracts, the kinetic energy of the mass particles closing in under the force of gravity is transformed into heat energy. Whereas this is still believed to be cosmologically the first cause of stellar radiation, it has been realized since the end of the previous century that the process of contraction would run to its end in a time that is short as compared to the age of the earth; and, ever since this was realized, astronomers have been compelled to postulate that the Helmholtz contraction must be retarded and, for the major part of the star, probably balanced by an internal pressure caused by energy that does not derive from kinetic energy. The way out of this difficulty was cleared theoretically (1905) by Einstein's law of the equivalence of mass and energy, although the exact mechanism whereby, under stellar conditions, matter would change into radiation, remained still a secret. The first laboratory transmutation of nitrogen into an isotope of oxygen by Rutherford (1917) opened the field of nuclear reactions which led to the experimental results; namely, that the mass lost in a nuclear reaction and the energy set free are in accordance with Einstein's law. By examining all possibilities of reactions that could take place under conditions prevailing in the sun, Bethe (1913) succeeded in singling out the one reaction that should, both as to the requirement of temperature and as to the availability of the elements involved, take place at the proper rate. This is the so-called carbon cycle whereby the energy liberated is equivalent to the mass defect of the helium atom as compared to four hydrogen atoms.

Prodigious progress has been made since the first A. Cressy Mor-

rison Prize on the above subject was offered in 1926. It is, however, felt that the complete answer has not yet been given and many questions remain open. In the first place, no final model for the sun, properly satisfying the observed luminosity and hydrodynamical considerations, has been published. Secondly, at best a start has been made on the problem of the so-called "red giants," "sub dwarfs," and "white dwarfs." Through the continued interest of Mr. Morrison and his desire to stimulate further research in the subject, the above prize will be renewed for award in 1946.

The following members were elected to Fellowship:

Reginald M. Archibald, Ph.D.
Ralph S. Banay, M.D.
John B. Bateman, Ph.D.
Erwin Brand, Ph.D.
Frank Brescia, Ph.D.
Dean Burk, Ph.D.
Wallace M. Cady, Ph.D.
Harry A. Charipper, Ph.D.
Albert Claude, M.D.
Christopher Coates
William P. Comstock, A.B.
Karl K. Darrow, Ph.D.
Thomas B. Drew, M.S.
Walter Dyk, Ph.D.
Gordon F. Ekholm, Ph.D.
William H. Eyster, Ph.D.
Karl A. Folkers, Ph.D.
Raymond F. Fuoss, Ph.D.
William U. Gardner, Ph.D.
Walther F. Goebel
Eli D. Goldsmith, Ph.D.
Manuel H. Gorin. Ph.D.
David E. Green, Ph.D.
Jesse M. Greenman, Ph.D.
Paul B. Hamilton, Ph.D.
Herbert S. Harned, Ph.D.
Harold J. Harris, M.D.
George Herzog, Ph.D.
Horace S. Isbell, Ph.D.
J. Brookes Knight, Ph.D.
Otto Loewi, M.D.
Sandor Lorand, M.D.
William A. Lynch, Ph.D.
Wilbur G. Malcolm, Ph.D.
Douglas A. Marsland, Ph.D.
A. H. Maslow, Ph.D.
Bela Mittelman, M.D.
Hans Molitor, M.D.
Ruth L. Munroe, Ph.D.
David Nachmanson, M.D.
Bernard L. Oser, Ph.D.
Benton B. Owen, Ph.D.
Edward A. Saibel, Ph.D.
Alexander Sandow, Ph.D.
Bobb Schaeffer, Ph.D.
Martin Scheerer, Ph.D.
Charles R. Schroeder, Ph.D.
Joseph E. Smadel, M.D.
William H. Stein, Ph.D.
Robert G. Stone, A.M.
Henry D. Thompson, Ph.D.
Oskar Paul Wintersteiner, Ph.D.

Honorary Life Membership was conferred upon the following eminent scientists:

Brönstedt, Johannes Nicolas, R, Ph.D., Physical Chemistry. Thermodynamics and Solutions, Acid Base Concept. Director, Institute of Physical Chemistry, Bledgansvej, Copenhagen, Denmark. R = Knight of Flag of Denmark (Highest honor awarded in Denmark). Visiting Professor, Yale University, 1926; Special Guest, Chicago World's Fair, 1933.

Drummond, Sir Jack Cecil, Biochemistry, Nutrition and Vitamins, particularly A and E. Professor of Biochemistry, University College, London, England, 1919–1939; D.Sc., University of London; Fellow of the Royal Society, 1944; F. R. I. C.; Lane Lecturer at Stanford University, 1933; Harvey Lecturer, New York City, 1933; Falarian Lecturer, Royal Institute, 1944. Appointed

to Ministry of Food, Chief Scientific Advisor, 1939, concerned during war period with planning of British Food Program also with plans for Relief of Western Europe on liberation. Knighted in recognition of this service. Attached to SHAEF and 21st Army Group for study of nutritional problems as France, Belgium and Holland liberated; Advisor to British Control Commissioner for Germany and Austria, 1944–1945; British Delegate to Hot Springs Conference, 1943; British Advisor at Food and Agriculture Organization Conference at Quebec.

Kapitza, Peter, Physics. Low Temperatures. Director, Order of Red Banner of Labor Institute of Physical Problems; Member, Academy of Sciences of U. S. S. R., Moscow, U. S. S. R.; Honorary Member, University of Algiers; Member, American Philosophical Society; Honorary Member, Franklin Institute; Honorary Member, Institute of Metals; Member, British Institute of Physics; Member, British Royal Society; Member, Cambridge Philosophical Society; Member, Société de Physique de France; Honorary Member, Moscow Society of Naturalists; Fellow, Trinity College, Cambridge, England. Awarded Franklin Medal (U. S. A.); Faraday Medal (England); Twice, Laureate of the Stalin Prize.

Oliphant, Marcus Lawrence Elwin, Physics. Poynting Professor of Physics, University of Birmingham, England; Messel Research Fellow, Royal Society, 1931, Fellow and Lecturer, St. John's College, 1934; Assistant Director, Cavendish Laboratory, Cambridge, 1935; Fellow of the Royal Society, 1937.

Szent-Gyorgi, Albert, Medical and Organic Chemistry. Professor of Medical and Organic Chemistry; Director, University Institute for Medical Chemistry, University of Szeged, Hungary; M.D., Ph.D., University of Hungary; Visiting Professor, Harvard University, 1938; Awarded Nobel Prize for Medicine, 1937.

Swellengrebel, Nicholas Hendrick, Parasitology. Chief, Zoological Laboratory, Amsterdam Tropical Hygiene Institute, 1913; Professor of Parasitology, Amsterdam University, 1921; Member, League of Nations: Malaria Commission, 1924; Public Health Council, 1927. Darling Gold Medalist, League of Nations Organization for Hygiene and Public Health; President, Third International Congress on Malaria, Amsterdam, 1938.

Umbgrove, Johannes Herman Frederick, Geology. Professor of Geology, Technische Hoogeschool, Delft, Holland; Ph.D., University of Leyden, 1925; Geologist in East Indies, 1926–1929; Assistant Professor of Geology, University of Leyden, 1929.

Wenyon, Charles Morley, Protozoology. Director, Welcome Research Institution, 1944; Formerly, Protozoologist, London School of Tropical Medicine; President, Royal Society of Tropical Medicine, 1945. Awarded, Cross St. Michael and St. George, 1918; Companion British Empire, 1919; Fellow of the Royal Society.

The following officers were elected for the year 1946:

For President

WALTER H. BUCHER

For Vice-Presidents

CHARLES H. BEHRE, JR. — ROSS F. NIGRELLI

EMILY T. BURR — JOSEPH S. FRUTON

GEORGE HERZOG

For Recording Secretary
CHARLES M. BREDER, JR.

For Corresponding Secretary
H. HERBERT JOHNSON

For Treasurer
HARDEN F. TAYLOR

For Librarian
ERICH M. SCHLAIKJER

For Editor
ROY W. MINER

For Councilors (1946–1948)
HANS T. CLARKE
CHARLES GLEN KING
HANS MOLITOR

For Finance Committee
W. REID BLAIR, *Chairman*
MARVIN D. THORN
ADDISON WEBB

After the Business Meeting, the following papers were read on the subject of:

"FOOD, NUTRITION AND WORLD POPULATIONS"*

NUTRITION AS A WORLD PROBLEM
BY
DR. FRANK GEORGE BOUDREAU
Director, The Milbank Memorial Fund

NUTRITION AS AN EXACT SCIENCE
BY
DR. OTTO ARTHUR BESSEY
Public Health Research Institute of the City of New York

THE FUTURE OF NUTRITIONAL SCIENCE
BY
DR. CHARLES GLEN KING
Scientific Director, The Nutrition Foundation, Inc.

* The papers presented by Doctor Boudreau and Doctor King are published in this number of the Transactions.

NUTRITION AS A WORLD PROBLEM

By
Frank G. Boudreau

Executive Director, Milbank Memorial Fund, New York, N. Y.

Food and nutrition have played major parts in determining the size and many of the characteristics of the world's population. They will continue to play major roles in the future. How the world's people are fed will affect the size of the population, its geographical and age distribution, its political complexion, its standard of living and even, to some degree, the issues of war and peace. Badly fed peoples do not necessarily decrease in numbers in this age of preventive medicine, but they do keep burning the fires of illiteracy, epidemics, social unrest and premature death. Food is so basic a need that even speculation about it may have serious consequences. Our farmers, farm leaders and agricultural economists anticipate the development of large surpluses in most of the major food commodities, as soon as the period of relief feeding is over. Action taken to prevent such anticipated surpluses (limiting production, for example) might well run counter to the requirements of a sound nutrition policy. For it is an ironic commentary on man's intelligence that the people have, as a rule, been least well fed when the largest food surpluses existed. The problem of feeding the unprecedentedly large world population that we expect in the future will not necessarily be solved by producing adequate amounts and kinds of food. We must also succeed in getting the food to the people. So far, however, we have been more successful in producing food than in properly distributing it.

The modern study of population problems may be said to have started with Thomas Robert Malthus, an English economist, who, in 1798, published his famous essay, "The Principle of Population as It Affects the Future Improvement of Society." Malthus assumed that man's sex passions were not likely to change and that food is necessary to man's existence. Population had always tended to increase rapidly and it would always increase rapidly, providing food or subsistence were available. According to Malthus, population would tend to increase to sixty-four times its original size, in 150 years (geometrical ratio), while its subsistence would increase only seven times (arith-

metical ratio). Accordingly, hunger, disease, vice and war were bound to be, in the future as in the past, the chief forces which would keep human population within reasonable bounds.

Benjamin Franklin had some influence on Malthus, for he is quoted in the first edition of the essay. Franklin's "Observations Concerning the Increase of Mankind and the Peopling of Countries," appeared in 1751. In this, he stated that the population of colonies would double every quarter of a century. In a curiously modern note, he also drew attention to the low fertility of the higher income groups: "The greater the common fashionable expense of any rank of people, the more cautious are they of marriage." Accordingly, Franklin advocated increase, Malthus, decrease in population. Franklin saw the great need for more people to develop the sparsely settled colonies, while Malthus was influenced by the squalor and misery of masses of people whose numbers had increased so greatly with the advent of the industrial revolution.

Let me survey briefly what we have learned about population growth, as a result of the stimulus furnished by Malthus and others of his kind.

Kingsley Davis[1] likens the growth of world population to a long thin powder fuse that burns slowly and hesitatingly, until it reaches the charge and explodes. For thousands of years, while man depended on hunting and fishing for his sustenance, the world's population remained exceedingly sparse. The first real burst of population growth came with the industrial revolution, which not only gave an unprecedented impetus to population growth in Europe, but extended its influence throughout the world.

"For the first time the world's entire population could be regarded as a single entity responding in various degrees to one dynamic process. For the first time the movement of human masses across oceans became feasible. For the first time a new type of balance between births and deaths—a balance less wasteful than the old—began to manifest itself."[2]

Even today, we do not know the exact size of the world's population, but students have been able to make reasonable estimates of numbers and rates of increase since the middle of the seventeenth century. Thus, in 1650, the world's population is estimated to have been

[1] **Davis Kingsley.** The world demographic transition. Ann. Am. Acad. Pol. Soc. Sci. 1945.
[2] *Ibid.*

545 millions. It grew to 728 million, in 1750; to 906 million, in 1800; to 1,171 millions, in 1850; to 1,608 million, in 1900, and to 2,171 million, in 1940. Rates of increase also continued to accelerate. The rate of growth for 1650–1750, 0.29 per cent annually, was the lowest for any like period in recorded history, and, in the following period (1750–1800), it nearly doubled. The rates for subsequent periods were, 0.44 (1750–1800); 0.51 (1800–1850); 0.63 (1850–1900); and 0.75 (1900–1940). These rates may not appear high, but, if the present rate were to continue, the population of the world in the year 2240, less than 300 years from now, would be 21 billion inhabitants.[3]

Just what caused these great increases in the world's population? Population growth is determined by fertility and mortality, through which all other factors must act. There is no reason to believe that there has been any significant increase in fertility in any considerable area of the world since 1650; there is evidence to show that fertility has actually declined. Hence, the cause of the growth of population must have been the decline of mortality. According to Kingsley Davis, "the reduction in mortality began primarily with a more abundant, regular and varied food supply. The indications are that the average expectation of life at birth has practically doubled since the late seventeenth century."

In the latter part of the nineteenth century, fertility, which had at first remained high, began to decline in the northwest of Europe.[4] This trend soon moved east and south, across the continent, meanwhile becoming well established in North America, Australia and New Zealand. "These are the only populations that have controlled their fertility sufficiently to bring birth rates into balance with the low death rates that modern conditions permit. They are the only populations that have thus far shown a way by which growth can be checked other than through death; the only ones that have attained high efficiency in the maintenance of the stream of life."

By 1940 or earlier, fertility in these countries had fallen below the level required for the permanent maintenance of a stationary population at existing levels of mortality. Their population growth continues only because of the present favorable age distribution, which time will soon alter.

[3] *Ibid.*

[4] **Notestein, Frank W.** Population—the long view, in Food For The World. Harris Foundation Lectures. University of Chicago Press. Chicago. 1944.

These countries are, therefore, classified by Notestein as those of *incipient population decline.* They are expected to reach their maximum between 1950 and 1970.

Populations in certain other regions are in an earlier stage of demographic evolution, but the decline in their birth rates is already well established. The populations of Eastern Europe, the Soviet Union and Japan, certain Latin American countries, Turkey, Palestine and parts of North Africa belong in this category, which is known as the stage of *transitional growth.* Japan is the most interesting example of this class, "for it is the only Eastern country that has gone through a substantial period of modernization and urbanization, hence the only one in which the demographic responses to these changes can be compared with those of the West."[5] Notestein points to the marked similarity between the trend of birth and death rates for Japan from 1921 to 1941, with those for England and Wales from 1881 to 1901. Populations in the stage of transitional growth increase enormously after fertility begins to fall, for it always lags behind the decline in mortality. Between 1821 and 1921, the population of England and Wales increased threefold, in spite of substantial emigration. If the population of Japan should follow the same course as in England after 1821, its population would probably approximate 95 million inhabitants, by 1970.

More than half of the world's population has not yet entered the period of transitional growth. Populations having *high growth potentials,* the third great division, are found in Egypt, central Africa, the Near East, Asia outside of Japan and the Soviet Union, the Islands of the Pacific and Caribbean, and much of Central and South America. The case of India throws light on the significance of high growth potential. From 1872 to 1921, periods of slow growth alternated with periods of rapid increase. There followed, for the first time in recorded history, two successive decades of rapid growth, and in those twenty years India's population increased by 83 million. It is now over four hundred million, about equal to the population of all Europe, west of the Soviet Union. These countries of high growth potential, typified by India, constitute one of civilization's most difficult problems.[6] If nothing is done for them or by them, they will, so far as we

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[5] *Ibid.*

[6] Many countries in this stage present no immediate problems, since they are sparsely settled and of slow population growth. The difficulty will come when the present very high mortality is brought under some control.

know, remain in the stage of high growth potential, periods of extremely rapid growth alternating with periods of slow growth or of decline, when famine and disease sweep through the population. In our modern world, however, we cannot do nothing, for our own safety demands action. We cannot allow disease to flourish unchecked in these lands, since the world is now so small that epidemics in any part of the globe threaten our own existence. We have learned that the only successful way to prevent epidemics is to deal with their sources, before the sparks have burst into flame. This means sanitation, a sound system of health administration, health education of the people, medical care and improved nutrition. Hence, if we attempt, as we must, to abate the sweep of epidemics, mortality will decline in populations of high growth potential and numbers will greatly increase. To do nothing and be forever faced with the problems of high growth potential, epidemics, misery and, probably, revolution; to do a little to check epidemics and be confronted by enormous increases of miserable peoples whose numbers more than keep pace with their increased food production; these are two of the choices with which modern society is confronted. Fortunately, there is a third choice: to assist these countries to advance socially, politically and economically, so that they may pass rapidly through the stages of population growth and attain, as some Western countries have done, a more reasonable balance between births and deaths. This, as I see it, is our only hope. "For it is only when rising levels of living, improved health, increasing education and rising hope for the future bring new value and dignity to the individual's life that old customs break and fertility comes under control. Without such control the world's population is limited only by its carrying capacity. In the long run it remains true that the control of mortality without the control of fertility is impossible."[7]

On the assumption of general order and the spread of modern techniques, Notestein believes that a world total of three and a third billion inhabitants, in the year 2000, is a conservative estimate. If, in the East, there should be a rapid increase in production within a slowly changing social framework, by the year 2000 an Asiatic population of well over two billions—about equal to the world's present population—would probably result. And a world population of three

[7] *Ibid.*

billions, within two generations, is not by any means a remote possibility.

This is a world problem, it cannot be dealt with unilaterally. Countries with populations in the stage of incipient decline are setting up commissions to study their problem. If experience is any guide, these committees will recommend measures designed to increase the number of their inhabitants. In the meantime, half of the world's population is in the stage of high growth potential, with all that this stage implies: periodic famine, such as we have witnessed recently in Bengal and China, high illiteracy, malnutrition and disease. All four are a menace to society throughout the world. In that half of the world's population which is in the stage of high growth potential, the expectation of life is about the same as that which probably prevailed in the advanced countries, prior to the industrial revolution. There is a shocking contrast between, for example, New Zealand and The Netherlands, where expectation of life at one year is over 65 years, and Egypt and India, with rates of 36 and 26 years, respectively. In 1938, some 12 per cent of the world's people lived in countries where the average expectation of life was 64 years or over, while 70 per cent lived in countries where the average span of life was under 50 and, frequently, well under 40 years.[8]

Turning now to another aspect of our subject, we may ask, Is it possible to produce enough of the right kinds of food adequately to feed the world's present and future population? Looking ahead only a few years, the problem is an entirely different one. It is whether we can maintain *food consumption* at a high enough level to prevent food producers from being impoverished by the very abundance of their production. Hunger and starvation are the lot of millions in the world today, yet the economist, looking into the future, sees food surpluses developing like those which ruined many of our farmers and our markets, in the 1930's. Wheat[9] is almost certain to be in a surplus situation, as soon as the relief feeding period is over, and this situation is likely to be chronic. Existing markets will find difficulty in absorbing the supply of fats and oils, when Pacific supplies become available

[8] Memorandum by **F. L. McDougall.** The Quantity and Physical Quality of Life in Relation to Poverty and Malnutrition. Prepared for the United Nations Interim Commission on Food and Agriculture. Washington. 1944.

[9] The following predictions are taken from an article by **Walter W. Wilcox.** Food supplies in prospect after the war, in Food For The World. Harris Foundation Lectures. University of Chicago Press. Chicago. 1944.

again. There will be a surplus in sugar, as soon as the present demand for industrial alcohol declines. With the expected increased supplies of animal protein, the demand for *pulses* is expected to decrease, and these will be in surplus unless production is adjusted. Rice is likely to be in a postwar situation similar to wheat. Fruit surpluses are likely to develop, in this country, and animal protein supplies will be in surplus, unless there is full employment. When military requirements cease, market surpluses will develop in vegetables. More food will be produced after this war than ever before in history, and market surpluses in a number of food commodities will appear at an early date.

In the face of these beliefs, which are shared by most food economists, we need not give much thought to the danger of future food shortages. For the Western world, at least, the gloomy predictions of Malthus have not been fulfilled.[10] World population has increased, but world food output has increased faster. The opening up of new lands and the application of scientific discoveries to farming have brought about a prodigious increase in agricultural output. For over two hundred years, man has produced more and more food with each succeeding decade. Mechanization and increased and better use of fertilizers and other technological advances have been put to work over a comparatively small fraction of the world. They are bound to be extended, in the future. The present increase in production is due to these and many other developments. The boundary of cropping has been pushed farther north in Canada and the Soviet Union, by the introduction of new cereal varieties and new vegetables. In India and elsewhere, hundreds of thousands of new acres have been opened up by irrigation. There has been a general increase in yields per acre, due to such advances as hybrid corn, disease-resistant varieties of cereal, new grasses and improved vegetables. New developments in the production and use of fungicides have reduced losses, especially in fruit and vegetable growing. There have been great increases in the numbers and productivity of livestock, through better breeding, increased understanding of animal nutrition and more effective prevention of animal diseases. And there have been great advances in farm and marketing equipment of all kinds, such as farm machinery, storage facilities, refrigeration, and transport.

[10] Report of the Technical Committee on Agricultural Production, in Five Technical Reports on Food and Agriculture, submitted to the United Nations Interim Commission on Food and Agriculture. Washington. 1945

There are still other opportunities to increase the world's food supplies. We know far too little about our resources in fish. Some 98 per cent of the world's catch is drawn from the Northern Pacific and Northern Atlantic Oceans.[11] Opportunities exist for expanding the fishing industry, in South America, Asia and Africa; the resultant production would improve diets and yield additional dividends in the form of vitamin oils. The food supply may also be augmented by cultivating fish in fresh water ponds. This would contribute needed protein to the diet of people in many parts of the world.

War experience has clearly revealed that better use can be made of the food produced in almost any country. During the war, we produced, in this country, enough food to feed a population one and a third times as great as our own. A Committee of the Food and Nutrition Board showed that, with slight changes in production and consumption, we could have fed a population twice as large as our own and actually improved the diets of our own people. All that was needed was more emphasis on the production of nutritionally desirable foods that are economical in land, manpower and transport, and the shifting of increased proportions of skim milk, wheat and barley, soy bean and peanut products, from the feeding of livestock into direct human use as food. Hence, by intelligent food management, the available world supplies of food can be made to go further and to promote better nutrition.

In the face of world food supplies, which appear to be more than adequate, what is the state of the people's nutrition throughout the world? We lack the data upon which to base any precise answer to this question, but we do possess sufficient information to draw some reasonably sound conclusions. Such conclusions were reached by a Committee of Agricultural, Economic, Nutrition and Health Experts, set up by the League of Nations, in 1935, to study both the health and economic aspects of the nutrition problem. Their report, issued in 1937, has probably done more to stimulate public interest in food and nutrition and to point the way for national and international action than any other contribution to the subject.

In the United States and Canada, said the Committee, much malnutrition existed among the lower income groups. Available data sug-

[11] Report of the Technical Committee on Fisheries, submitted to the United Nations Interim Commission on Food and Agriculture. Washington. 1945.

gested a figure of between 20 per cent and 30 per cent of the entire population. Information on nutrition in Great Britain was available in a remarkable report by Sir John Boyd Orr, *Food, Health and Income,* which appeared in 1936. Applying the results of an extensive survey to the entire population, the report concluded that, in England, the average diet of the poorest group, comprising 4½ million people, is, by the standard adopted, deficient in every constituent examined. The second group, comprising 9 million people, is adequate in protein, but deficient in all the vitamins and minerals considered. The third group, comprising another 9 million, is deficient in vitamins and minerals. Complete adequacy is almost reached in group four (9 millions), while, in groups five and six (the wealthiest groups comprising 13.5 million people), the diet has a surplus of all constituents considered. The Committee pointed out that, in the British Dominions, although food consumption was relatively high, malnutrition was not uncommon, especially among children. In Central and Eastern Europe, malnutrition prevailed extensively and there was often a lack of staple foods as well.

The Committee agreed that the diets of the great mass of the population in the East were grossly deficient in terms of any standards of adequacy put forward by nutrition workers, and that nutrition was of far greater importance to the people of the East and to Eastern health workers than is the case in the West. In Asia and the tropics, about 75 per cent of the 1,150 million inhabitants consumed diets far below the standard for health.

This outline of the nutritional status of the world's people has been greatly supplemented, since 1937, for the League's work stirred the Governments to new activity. All of the new evidence, from whatever source, has served to emphasize and accentuate the picture drawn by the Committee. Famine in China, The Netherlands East Indies and India, and the serious aggravation of malnutrition in Europe, due to the war, make it plain that we have a long way to go before we can be satisfied that the larger part of the world's population enjoys a diet which contains even a bare minimum of the nutrients essential to health.

I have already suggested that the worst diseases have their chief source and habitat among populations having the highest fertility rates. It is not surprising to find that, as a rule, the worst and most extensive

malnutrition is also found in population groups with the highest birth rates. These people inhabit the less advanced countries; their expectation of life is low, because of the mortality which takes a high toll in every age group, but particularly at the younger ages. In India, nearly half of the total mortality occurs in children under ten years of age, a percentage about five times greater than that of Western countries.

Perhaps I have not made it plain that there is no geographical division of the world into two parts; one part enjoying relatively low death rates, low fertility and, at least, moderately good nutrition; and the other having high death rates, high fertility and great malnutrition. The world's *population* may be divided roughly into these two parts, but even the richest countries, among their poorer classes, have large groups with relatively high fertility, death and malnutrition rates. Taking the standardized death rate for the United States, as whole, at 100, the figure in 1940 for North Dakota was 60.2, for Iowa 79.6, for Massachusetts 96.3, for Mississippi 114 and for South Carolina 125.

The picture I have drawn for you is a dark one, but there are many gleams of sunshine on the horizon. The war has taught us that the right kinds of food can be produced and transported to supply the needs of the nation or a group of nations, even when war interposes seemingly insuperable obstacles. During the war, far more food was produced than we would have thought possible in prewar years. Our food supplies were managed in such a way as to conserve land, manpower, transport and critical materials. They were distributed more efficiently than ever before in history, with emphasis, especially in the United Kingdom, on the physiological needs of the people. Among the Allies, the food situation was most acute in the United Kingdom, and it was there that the greatest efforts were made to feed the people adequately. What the results have been is told by Sir Wilson Jameson, Chief Medical Officer of the Ministry of Health, in a broadcast message to his people in October, 1944:

> After five years of war we still have a good story to tell. The most sensitive index of a nation's general health is probably the proportion of infants dying in the first year of life. In the last war it rose steadily. During the last three years it has declined steadily and, last year, was the lowest ever recorded. The most risky time for a baby is its first month of life. Well, we've got a new low record there; and as for the tragedy of babies born dead (stillborn as we say) I can tell you that the chance of this happening is only three-fourths of what it was five years ago. The death rates for children up to ten years of age were last year the

lowest on record, as was also the proportion of mothers dying as a result of their confinements. As the war has gone on, the vital statistics for mothers and children have continued to improve and in the fifth year they're the best we ever had. This can't be just an accident. All that's been done to safeguard mothers and children must have had some effect—such things as the national milk scheme, vitamin supplements for mothers and children, the great extension of schemes for school meals and milk in schools. There are doubtless other factors—full employment and higher purchasing power in many families, especially in the old depressed areas; as well as the careful planning from a nutritional point of view of the restricted amount of food available for the nation.

Advances in science have provided us with materials with which to build a new world. An essential condition of success is that the peoples of the world must work together in building it. Otherwise, it will be a house of cards. After the first world war, the different countries attempted to retire behind their national frontiers, building up social, political and economic walls to keep out foreign ideas, foreign goods and foreign influences. The worldwide economic depression, followed by the war, revealed the folly of attempting to break up the world into tight compartments. There was no chance of winning the war, until full cooperation developed among the Allies. It is pertinent to our subject to recall that food, as a weapon of war, was planned, produced, processed, transported, allocated and distributed according to plans worked out by joint committees of experts from the allied governments.

We are attempting to continue joint food planning, by means of two United Nations agencies: UNRRA and FAO. UNRRA is for the short term. I am convinced that UNRRA will go down in history as a magnificent conception of farsighted men who understood that, in the modern world, it is the part of enlightened self interest to restore neighboring nations to decent conditions of living and work. FAO, which was established a few weeks ago at Quebec, is for the long term. By setting it up, the Governments have agreed not to go back to the methods and ways of the interwar period, but to continue, in peacetime, the cooperation in food and nutrition which helped to bring victory in the war.

The first conference at Quebec selected Sir John Boyd Orr of Aberdeen as their Director General, and he thus became the first full time official of the United Nations. Sir John, as many of you know, won his spurs in animal nutrition. He proceeded to build up a reputation in human nutrition, and he is known, throughout the world, for his interest and concern for the hungry and malnourished among the world's people. The selection of Sir John Orr was proof that the Governments

were sincere in their resolve to work together to raise the standards of living of their people and to banish hunger and want from the world. The conference at Quebec was a sober gathering of technical experts and responsible statesmen, fully aware of the difficulties and complexities of their task, appreciating that the many-sided problems of poverty and malnutrition required many different forms of attack, by many United Nations agencies, in trade, finance, industrial development, education, transport and politics, and resolved to do their part so well that they would set an example to all the others.

Looking back on the history of our own country, I do not find that our difficulties and troubles were ever solved, more than temporarily, merely by the strengthening of police powers. On the contrary, progress has been due to our success in developing our natural and human resources, by such things as wages and hours and other social legislation; by the development of our system of education; and by the building up of institutions and policies which permitted the human spirit to grow and flower. The world today—physically, socially, economically, and politically—is smaller than the world Washington and Jefferson knew, when they labored to lay the foundations of this republic. If the advance in science is to prove a blessing and not a curse to mankind, we must think in world terms and build up for our world the social, economic and political institutions which will set mankind on the road to freedom from the misery, want, disease and premature death which have been man's companions for so many centuries. As the member from New Zealand said at Quebec, when the Food and Agriculture Organization was established:[12]

> At this vital time the opportunity is given to few organizations to contribute more directly to human welfare, and, in so doing, to make more permanently secure the foundation upon which world peace must ultimately rest; for neither political alliances nor military commitments, nor atomic bombs, nor regional arrangements can guarantee security for long in a world that is afflicted with poverty, pestilence and famine.

[12] Journal of the First Session of the Conference of the Food and Agriculture Organization of the United Nations 1 (4): 40. Quebec City, Canada. 1945.

NUTRITION AS AN EXACT SCIENCE

By

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(Illustrated)

Dr. Bessey and his associates have developed remarkable techniques of direct quantitative measurement of the nutritional state of individuals by micro-chemical analysis of a few drops of blood. These results he presented, together with the clinical signs of malnutrition and their statistical application to school-children and other groups, as examples of exact science in the advancement of nutrition.

THE FUTURE OF NUTRITIONAL SCIENCE

By

Charles G. King

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The two previous speakers have made it clear that the future of nutritional science is not only going to be woven into the complex fabric of other sciences, but it is destined also to play an important role in social and economic progress.

The primary objectives that nutritional scientists see ahead are not difficult to state, but they are undoubtedly going to be difficult to achieve: We want to find what quantity of each of the forty or more essential nutrients is required for good nutrition, how each nutrient functions inside the body, how each can be used to protect human and animal health, and how each can be measured accurately, either as it functions in the body or as it comes into being on a farm and later takes its course through the channels of commerce.

This goal may not sound formidable, but I think it is safe to suggest that if everyone in the audience had a Ph.D. or M.D. degree and could have as many research assistants as desired, plus an opportunity to work for a hundred years without interruption, there would still remain large segments of the goal to be achieved. In other words, Dr. Vannevar Bush rendered a valuable public service when he entitled his recent book "Research. The Endless Frontier."

Few people realize how limited our present knowledge is, in regard to human nutrition. For example, we still do not know what chemical elements and compounds must be supplied to the human body to meet its basic requirements for growth and health.

Most authorities would agree that at least 19 elements are needed by some forms of life, but the evidence scarcely goes beyond 10, in relation to human feeding.

Of the 23 common amino acids that constitute our protein foods, it appears that only 8 are required by the adult human. However, we know almost nothing of special requirements for other conditions, except that of maintaining a nitrogen balance in a normal healthy adult. Animal tests give only a very rough indication, because each animal tested, thus far, shows distinct variations from the others.

Of the 15 or more vitamins that have been discovered by animal experiments, the tests in human feeding cover only 9, clearly, and three others, in part. It is worth noting, however, that, up to the present time, the human has not been found capable of synthesizing a vitamin required by any test animal.

With regard to fats, although a few animals appear to need one of the 43 or more fatty acids, practically nothing is known of the human need for fats, other than for three of the fat-soluble vitamins.

Neither is there any clear picture of the human need for carbohydrates, although they normally supply about one-half or more of all human and animal energy.

At first thought, one might be critical of the current expenditure of funds to find how the individual nutrients function in living cells, but we are confident that this field lies at the very heart of understanding other aspects of the science. If scientists can find out what the nutrients do inside living cells, including the human body, then agriculturists, food technologists. physicians, dietitians and public health workers will be in a position to produce, distribute and use foodstuffs on a far more rational basis. Without such information, men are forced to make important decisions on the basis of rule-of-thumb or guesswork, and that is not good. It is, certainly, the extreme opposite of what this Academy stands for in community life.

There is always a temptation to indulge in a bit of crystal-gazing, when one is looking into the future, but I think we get our best clues when we join with our fellow scientists in thinking out loud. We start, then, from the current zone of experimental evidence and visualize our next moves within that discipline.

The normal, adult, human requirement for each of the amino acids supplied in such protein-rich foods as milk, eggs and meat will soon be known. There will still remain such questions as:

(a) What are the special requirements during growth?

(b) What are the special requirements during pregnancy and lactation?

(c) What are the special requirements during convalescence from injury and infection?

(d) What are the special requirements to build up maximal resistance against infection?

(e) What are the special requirements to permit optimal adjust-

ment to extremes of environment such as cold, heat, high altitude and restricted water intake?

(f) What are the upper limits to a satisfactory protein intake? Nutritionists want to know the approximate optimal range for each nutrient, as well as the critical levels.

(g) And to what degree do each of the above six items vary with differing types of proteins?

As these functional needs become known, in terms of the consumer, the medical profession and the respective areas of agriculture, education and the food industry will be in a position to adequately advise and supply the public. The essential or desired amino acids, for example, can then be obtained, at the discretion and pleasure of the consumer, as caviar, or filet mignon, or just plain bread and milk. I see no serious threat that the public will prefer two pounds of pills and a bottle of distilled water to ordinary food. The synthetic amino acids and protein hydrolysates will, of course, come into their proper areas of use, in time. So far as the distilled water is concerned, I have not been able to observe, especially since coming to New York, that the consumption of distilled material is based on water content at all.

Despite the very great importance of fats in animal physiology and in industry, very little is known regarding even such simple things as their comparative nutritive quality, or the chemical changes by which they are formed, or the chemical steps by which they are used in living cells. Until very recently, one might say with complete fairness, the literature on fats afforded a critical scientist very "lean pickings." It is interesting, also, to note that a considerable portion of the more promising recent work on fats, such as that of Dr. Stettin and his associates at Columbia University, stems chiefly from the study of carbohydrates.

The chemistry and physiology of sugar utilization has been moving ahead rapidly in recent years. The group associated with Dr. Clarke and the late Professor Schoenheimer at Columbia University has been very productive, as many of you know. At Washington University, Dr. Carl Cori has recently published a short paper that augurs many important findings. Having previously isolated several of the enzymes that control by catalysis the use of blood sugar, Dr. Cori's group then showed, for the first time in history, how two of the hormones function in terms of their molecular reactions.

This remarkable work of Dr. Cori's group, taken in conjunction with progress in other laboratories where the chemical aspects of diabetes are under investigation, gives one great encouragement. Medical authorities believe that one person in every one-to-two hundred has a metabolic disturbance of diabetic type. It seems very reasonable to hope that, with further research, the cause or causes of diabetes will soon be identified. Then, having identified and understood the cause, those who work toward better public health should be able to develop techniques and educational measures to meet the challenge of preventing the onset of the disease. Insulin has been a great boon to humanity; but to prevent diabetes would be a far greater one.

Of the many reports relating food intake to health, few have received so much attention as the recent papers from Harvard University. Dr. Harold Stuart, Mrs. Burke and their associates reported summary findings, after 12 years of careful study. The diets of mothers living in wage earners' homes in the Boston area were classified as to nutritive quality and these records were later correlated with the health records of the mothers and their infants. If the correlation between food intake and health over the rest of our country should be shown to be even remotely like that reported by the Harvard group, the appeal to do something about it would be irresistible. It would mean an improved initial health rating for approximately 900,000 infants per year in the United States. To provide more information in this field and to serve as a stimulus to education in nutrition, several projects like the Harvard study should be under way in different areas of this country and in others. Furthermore, such studies should be extended, in time, to cover the whole life span, as Dr. Sherman has done in his experiments with albino rats. The cumulative effects of consistently good or bad diets have given such striking records with albino rats, under controlled conditions, that there is no sound basis for doubting their analogy, in a broad sense, to human feeding. Nevertheless, we critically need the scientific evidence, obtained and interpreted by persons with the highest professional standing, and as applied specifically to the *genus homo.* With apologies to our latin scholars, one might say, regarding the human situation, that, instead of having good data from which to draw conclusions *"ex-post facto,"* we are still forced to reason too largely *"ex-post albino."*

A promising project in the public health field has been initiated at

the Mayo Clinic, in cooperation with the local health department, but it has very different features than the work outlined by Dr. Stuart's group in Boston. One aspect of the Rochester plan is especially commendable, in that it anticipates a long period of observation.

Additional plans for maternal and infant nutrition research offer special promise. These projects illustrate the constant need to keep human nutrition research following as closely as possible along the lines indicated by findings with experimental animals. For example, Dr. Warkany's work with albino rats shows beyond dispute that deficiencies of vitamins A and B_2, during early pregnancy lead to frequent physical deformities in the offspring. The deformities often affect the eyes, the feet, the vertebrae, the jaws (so that they are too small for good tooth development); and occasionally Dr. Warkany finds conditions resembling "clubbed feet" and "cleft palate." A closer check of such possible relationships can be secured by studying monkeys or other primates, because their anatomical changes and nutritional requirements are so nearly human. Physicians will then be in a better position to look backward into the history of an individual patient who is the victim of a deformity, and, very possibly, identify the cause. Then, by education alone, there would be an opportunity to ward off many bitter human experiences. Dr. Warkany has estimated that human physical deformities at birth result in more deaths per year in the United States than are caused by diphtheria, whooping cough, scarlet fever and measles combined.

The subject of dental caries, or tooth decay, reaches into the personal experience of nearly everyone. Furthermore, there is reasonably good evidence that the incidence of dental caries has been rising, during the past two decades. It is now stated to be the most prevalent disease in America. Until they lowered the standards for Selective Service acceptance in World War II, it was the most frequent single cause for rejection on physical grounds—actually reaching over 20%, among the first two million called. Since it reaches so far down in the younger age groups, perhaps it should not be classed as a degenerative disease, but it does represent, in most cases, a premature decay of a mature tissue. There is no need to quibble over the question of whether oral environment, such as the presence of bacteria, sugars, and acids, may affect the degree of tooth decay, but I think there is a high probability that, in time, it will be found that good or bad food

practices are responsible in major degree for the health and stability of a mature tooth.

Other diseases that we may expect to give way, chiefly in the sense of postponement or decreased rate, when good nutrition is maintained, are certain types of heart failure, high blood pressure, aging of the skin, intestinal disorders, anemias, some types of liver and kidney diseases and of visual impairment. You will note that most of these diseases are chronic in type, and that, in so far as they are related to nutrition, one must study them in a perspective of many years.

There is little doubt, in the minds of those who are close to the current research frontier, that nutrition is destined to play an increasing role in regard to public health, preventive medicine, agriculture, food processing and distribution, lay education and the food habits of the general public.

In the pure research field, then, one can look forward to seeing an endless frontier of exploration into the mysteries of what happens inside a living cell. From the single cell, a gradual extension can be made to the human body. It is quite as clear, also, that research and guiding principles in the science of nutrition will include increasing emphasis upon the concept of a full life span and secondary effects upon succeeding generations.

JOHN ARCHIBALD WHEELER, Princeton University: *Polyelectrons.* (A. Cressy Morrison Prize Winner, 1945.) (Abstract.)

This paper is an extension of our knowledge about the interaction of positive and negative electrons. Already available experimental and theoretical evidence has shown that pairs of these particles may be produced from a vacuum by the action of electromagnetic radiation, and, conversely, that a pair consisting of an electron and a positron may undergo annihilation with the release of two quanta of electromagnetic energy.

The present paper shows that, preliminary to such an annihilation process, the electron and positron may circulate about each other in orbits closely resembling those of the hydrogen atom. The entity so constructed is given the name of bi-electron and is calculated to have a lifetime of 10^{-6} second or 10^{-10} second, according as the spins of the positive and negative electrons about their own axis are directed in the same direction or in opposite directions. It is shown that similar systems may exist with more than two electrons. Also, figures are given for the probability of creation of such polyelectron systems from a vacuum by the action of electromagnetic radiation.

The properties of these polyelectrons are compared with those of the cosmic ray mesons. The two types of system resemble each other in having lifetimes against radio active disintegration or annihilation of the order of magnitude of a microsecond and in having masses which are multiples of the electronic mass, but are distinctly different with respect to calculated penetrating power and geometric extension or size. An experiment is outlined by which it should be possible to check the theoretical prediction that the two quanta of electromagnetic radiation, given off in the process of annihilation of an electron and a positron, are vibrating in planes perpendicular to each other.

It is concluded that the study of polyelectrons is a logical step in the further development of the theory of electrons and positrons, and in the application of this theory to the study of cosmic ray problems.

MAX J. HERZBERGER, E. Kodak Research Laboratories, Eastman Kodak: *The Limitations of Optical Image Formation.* (A. Cressy Morrison Prize Winner, 1945.) (Abstract.)

The aim of this paper is to familiarize the optical designer with a new and powerful tool for the investigation of the image-forming qualities of an optical system.

The rank of the matrix, containing the second-order derivatives of a function, called the diacharacteristic, discloses the quality of the image obtained, whether sharp, unsharp symmetric, or unsymmetric. Thus, the lense-designer can compute the characteristic function of ray-tracing; by examining diagrams, he can determine whether the points in which he is interested have any of these qualities.

This method, which is of even greater value in the theoretical field, is here utilized in investigating the limitations of optical image formation. Answers are supplied, in particular, to such questions as: Do there exist systems which image the whole space sharply, or at least symmetrically? How many surfaces can any one image sharply?

Many classical examples demonstrate that the diacharacteristic permits straightforward solution of optical problems by the manner in which a system images any point. However, the way to a fundamentally different treatment—as, for example, in the analytical expression of errors of distortion—is pointed out by this technique.

The geometry of diapoints is discussed, as well as their coordinates expressed as various functions. The laws of image formation are examined, both for a finite and for an infinite object point. Finally, the characteristic functions for several interesting, special kinds of image formation are found, and the usefulness of the characteristic function to the investigator is plainly demonstrated.

LEONARD G. WORLEY, Department of Biology, Brooklyn College: *The Golgi Apparatus—an Interpretation of its Structure and Significance.* (Honorable Mention, A. Cressy Morrison Prize Competition, 1945.) (Abstract.)

The configuration in fixed animal tissue, to which the term "Golgi apparatus" has been applied by cytologists, is the manifestation of a series of more or less spherical Golgi bodies of the living cell. These bodies are believed to be the visible expression of the more or less temporary aggregation of ultramicroscopic colloidal particles, probably including phospholipids, ribo-nucleic acid or ribo-nucleoprotein and, frequently, ascorbic acid.

To this basic chemical structure are added, when available, through absorption from the cytoplasm, various raw protein and fatty materials. Since the proportion and character of these raw materials shows considerable variation in different cells and probably even within the same cell at different times, the Golgi elements show a correspondingly great variability in their total chemical composition. This is reflected in differences in structure, staining reaction, degree of osmiophily, viscosity, specific gravity, solubility and the nature of the Golgi body product in various situations.

The basic Golgi substance converts these raw materials into various products of a protein and fatty nature. It is believed to be directly responsible for the formation of all secretory granules, whether these are to remain within the cell or are destined to be discharged, as in the case of glands. Aggregation of the colloidal particles precedes product formation and the release of the product is usually accomplished through the partial or sometimes complete dispersal of these particles.

New Golgi elements may arise from pre-existing Golgi bodies, but new particles are believed to be cast into the cytoplasm, from time to time, by the nucleolus, especially during oögenesis. The phospholipid fraction may become incorporated after the particles of nucleic acid reach the cytoplasm. Mitochondria are believed to have a somewhat similar origin, but are thought to be relatively more stable, once formed, and probably also more constant in chemical composition.

Studies of the Golgi apparatus should always involve a comparison between fixed and living tissue. No structure in the cytoplasm of

either the vitally stained or the fixed cell should be accepted unless there is fairly perfect correspondence between the two types of material. In general, structures that blacken with prolonged osmication and stain vitally with methylene blue may be considered a part of the total Golgi complement of the cell. But, in the final analysis, judgment should be based, wherever possible, on a study of these inclusions through the entire cycle of activity.

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SECTION OF BIOLOGY*

JANUARY 14, 1946

DOCTOR ROSCOE R SPENCER, Chief, National Cancer Institute, Washington, D. C.: *Carcinogenesis and Cell Adaptations.* (This lecture was illustrated by lantern slides)

During the past five years, studies have been made of the action of carcinogenic agents upon free-living single-cell species, based upon the theory that the process we call carcinogenesis and the process by which such organisms become adapted to unfavorable environments may have certain features in common.

Essentially three concepts are involved in this theory:

1. That the cancer process is, in part, at least a *special type* of cellular adaptation to various unusual environments. Not all cell adaptations end in cancer. Reptiles and amphibians rarely respond to carcinogenic agents. Wooley, Fekete, and Little[1] have shown that cancer of the adrenal cortex may be induced by removal of the ovaries and testicles of new born mice of the Ce strain, but not in other inbred strains.

* No meeting was held in January by the Section of Geology and Mineralogy.
[1] Science **97**: 291 1943

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2. That the transformation of a normal cell to a cancer cell involves a step-like process. There is no clinical or experimental evidence to support the assumption that the genesis of a cancer cell involves a sudden change in a normal cell. On the other hand, the clinically observed association of cancer with chronic irritation; the various precancerous lesions and stages; the prolonged incubation period in all experimentally induced cancers; and such fundamental observations as Earl's[2] *in vitro* transformation of a single clump of normal cells to cancers of six or seven different types of behavior, all seem to fit in with the concept of *gradualism* in the genesis of cancer.

3. That the cancer process is basically a survival mechanism. It has been pointed out that the universal goal of living things is survival, but the survival of the cancer cell is a thing apart from the survival of the organism as a whole.

The successful immunization of individual animals to multiple lethal doses of various toxins is well known, and can be achieved best by means of interval injections of increasing doses over a considerable period. Naturally, one suspects that the permanent and successful adaptation of a race or species, in contrast to the adjustment of the individual cell or organism, would require a much longer time. However, it is believed that the underlying biochemical mechanism of species adaptation must, of necessity, be basically similar to the mechanism of adjustment of an individual organism.

A cancer cell is conceived of as a descendent of a normal cell that has gradually adjusted successfully over many cell division cycles or generations to an unfavorable environment. The adjustment is successful from the frame of reference of the cell. It is unsuccessful from the frame of reference of the organism as a whole, since the cancer cell becomes parasitic upon its host.

Our studies have yielded results which have suggested that there are several important principles involved in the mechanism by which cells become adapted to unfavorable environment.

Experimental data were presented by means of lantern slides which support the following tentative principles:

1. The *continuous* exposure of actively multiplying, free-living, bacterial species to an unfavorable environment may not be fatal to

[2] J. Nat. Cancer Inst. 4: 131 *et seq.* October, 1943.

individual organisms or cultures for a number of generations or cell-division cycles, but in due time, the species will die.

2. By discontinuous or alternating exposure, an actively multiplying species can adapt and continue to survive in an environment that is fatal when the exposure is continuous.

3. The ability of an organism to resist an unfavorable environment is a function of its age or maturity.

4. Organisms can resist higher intensities of an unfavorable environment during the resting stage than during the actively multiplying stage.

The belief is expressed that a continuation of these studies will lead to a better understanding of the underlying principles and mechanism concerned in species adaptation.

SECTION OF PSYCHOLOGY

JANUARY 21, 1946

DOCTOR T. C. SCHNEIRLA, Associate Professor of Psychology, Washington Square College of Arts and Science, New York University; Associate Curator, Dept. of Animal Behavior, American Museum of Natural History, New York, N. Y.: *Ants and Men: Problems in the Bio-Psychology of Social Organization.* (This lecture was illustrated by lantern slides.)

(An abstract of this paper will be published in a later issue of the TRANSACTIONS.)

SECTION OF ANTHROPOLOGY

JANUARY 28, 1946

DOCTOR EARL W. COUNT, New York, N. Y.: *The Evolution of the Race Idea in Modern Western Culture during the Period of the Pre-Darwinian Nineteenth Century.*

I.

For several centuries, we of European stock have been adjusting ourselves to the fact that the world is round and that there are humans on the other side of it. This does not mean that, until Columbus discovered America, we had no notion of other racial types. We have long been used to the idea that there were two kinds of people,—Christians and heathen, with the Jews constituting some kind of *tertium quid*. Ethiopians, black Pygmies, dark Easterners, sallow horsemen out of the steppes, were regarded sometimes as curiosities, sometimes as highly undesirable intruders. The less they resembled ourselves or acted like us, the longer it took us to recognize them as belonging to the human species.

However, when we began to explore the whole world and seize possession of it, a readjustment was necessary. In the beginning, circumstances required that the rest of mankind adjust itself to us; but, as a great secular wave of history recedes today, it is we who are compelled to adjust ourselves to the rest of mankind.

The history of the concept of race belongs to the intellectual side of that adjustment. The concept belongs to science. Now, science is a phenomenon peculiar to European culture in recent centuries. While the sciences may be pursued by their specialists severally, any study of the entity of *science* reveals it as an organ within the body of our culture, having its own physiology and participating in that of the whole body—both affecting that body and being affected by it.

However, before we probe further, let us take note of two things. One is the phenomenon of *culture lag* within science. Stated in epigram—Newton has been more important to biology than Darwin has been to physics. Since the Renaissance, we have inverted our strategy of exploring the universe; so that, instead of starting with man and

fitting the universe to him, we have been trying to do the opposite. Hence, what the physicist finds at the close of the seventeenth century may take another century and a half to permeate biological thinking.[1] I must hasten to add that this is meant as a description of a historical happening, and not as a statement of law. If it actually represents a law, then that is something still to be verified.

The other preliminary observation applies to a certain difference which apparently inheres between the physical sciences, on the one hand, and the biological on the other, and, still more obviously, between the biological and the social sciences. Physical science is what I shall call—I hope I am not thereby misunderstood—a "leader" science, while biological and social sciences are "follower" sciences. That is, physical science can possess a certain detachment which the others cannot. It explores an infinite universe and its findings, which do not depend upon human events, modify human activity. Thus, the discovery of a physical law can, for instance, eventually alter the material basis of living and also affect the attitude of the average man towards the universe. Biological science, on the other hand, deals with only a small part of the universe. Furthermore, it is to a considerable degree based on physics and chemistry. The converse of this, however, is not true. We narrow our field even further when at last we reach the social sciences; for, in spite of ants, wasps, termites, and *Rhesus* troops, the social sciences obtain their material essentially from the events of man only.

It is necessary to consider these things, because they have greatly influenced the evolution of race concept. Let us return to the position that raciology is the result of our Occidental culture endeavoring to adjust itself, one way or another, to the fact of the human species. It was the events of human history that compelled some sort of adjustment. We must expect, therefore, that raciology, as we conceive it, was engendered by the heat of natural aspirations in a way unknown to the physical sciences. It is possible, for instance, to consider almost the entire nineteenth century as one era of classical physics, starting with Count Rumford's heat experiments (1798) and ending in 1896, when Roentgen discovered X-rays and Becquerel, radioactivity. How-

[1] In the mid-eighteenth century, Lagrange created the calculus of variations. At the present time, this powerful tool, which would be invaluable to the metrics of man, is still practically unapplied in physical anthropology.

ever, this[2] we cannot do with raciology. It moves *with* history. The Napoleonic epoch brings to a close the era of the philosophers in science. Europe then enters upon a reactionary period and a rapid evolution of nationalism which profoundly influence the science of race. The first half of the nineteenth century is one of gestation in biological thought. Then, in mid-century, comes the explosion of Darwinism,[3] simultaneously with a climax in political life. At this time, anthropology is born. Its gestation has been conducted jointly by the life of general biology and of social history. Soon we shall find that even *social* history has affected the concept of *biological* race.

Apropos of social history as a factor—we quite understand that it has been responsible for *racism.* Racism is a product of nineteenth-century nationalism, although its antecedents are older. But raciology, too, the actual *bona fide* science, has been promoted and enriched by the developing self-consciousness among those culture-units of Europe, called nations. Let us distinguish carefully. Raciology is part of the honest attempt of scientific anthropology to understand man. Racism is a prostitution of race facts, plus a marshalling of pseudo-facts, fancies, and prejudices, to promote nationalism along pathological channels. Therefore, I shall do no more than mention, at this time, the *cults* of Indo-Germanism, Aryanism, and the "Pan-" movements, such as Pan-Germanism, Pan-Slavism, Pan-Turanianism, Pan-Hellenism, etc. In a comprehensive history of race thought, they have their place; for the pathologies of science are real and part of its case-history.

This brings us to the second great adjustment European man has been making to man; but this is an adjustment to himself primarily, not to world-wide man. Eventually, to be sure, the two adjustments must be enmeshed; but we need not complicate the story with this phase. After the fall of Napoleon, the European nations enter upon an unprecedented self-appraisal. They reach down into the bases of their composition. So they discover, among a number of other things, that they are a mixture of "races." Hereby, the word "race" is seen

[2] This again was a historical observation, and not a generalization, in the nature of a law. The great social events of the nineteenth century—for instance, the American Civil War—do not force physics into some hyper-activity, as the Second World War could do. The inventions of the nineteenth century, so necessary to the efflorescence of technology, did not wait upon the physicist as they now have to do. Nor was technology, then, the cultural giant that it is today. In other words, technology had not progressed as far in capturing physics as it has today. Since, therefore, technology is both a creator of cultural events and is much stimulated by cultural events, such as war, physics may yet end up by riding the waves and troughs quite as inescapably as anthropology. But, if and when this happens, it will not mean at all the same thing about the "soul" of physics that it will about that of anthropology.

[3] Darwin's hypothesis was forced out of him reluctantly, by the pressure of a cultural movement.

to take on a second meaning, one which leads eventually to much confusion. Obviously, I cannot be using the word on the same scale, when I speak of "Alpines" and "Mediterraneans," as when I speak of "Negroes" and "Mongoloids."

Let us divide the history of raciology into the following eras:

1. The eighteenth century, ending with the Napoleonic interlude;
2. The pre-Darwinian period, from the fall of Napoleon to about 1860;
3. The post-Darwinian period, from 1860 to the First World War;
4. The twentieth century, since the First World War.

For brevity's sake, we shall not go back to the eighteenth century, except incidentally; and, of the four periods, only the second—the "gestation" period—will be developed. The last two will only be mentioned. As a further limitation, we shall concern ourselves mainly with France, England, and Germany. For, with due acknowledgment of the greatness of other nations, these three lead Europe, at least in the *bulk* of nineteenth-century thought. If France, in our discussion, seems to receive more attention than the other two leaders, it is because, in the particular subject and period that we are dealing with, she is receiving her due.

II.

In preparation for the period we are to examine more closely, the least that we can say of the eighteenth century is this: It was what Merz has called the century of philosophy, as the sixteenth was that of the Reformation, the fifteenth that of the Renaissance, and the nineteenth that of science.[4] I think you will agree that it was the great breeder of ideas. It was molded, either posthumously or contemporaneously, by Newton, Leibniz, Spinosa, Johann Sebastian Bach, Handel, Haydn, Montesquieu, Diderot, the Encyclopedists, Voltaire. Its highest achievement in thought is probably Kant's "Critique of Pure Reason." The century was Deistic. It sought a rational universe into which rational man could fit. Linneus's orderly arrangement of animals, plants, and minerals must be understood from this

[4] *A History of European Thought in the Nineteenth Century.* Vol. I.

standpoint. It is the naturalist's version of the philosopher Leibniz's gradation of all animate entities which he led from the lowest life, through man, to God. It is entirely fitting, in fact, that the greatest thinker of the century, Immanuel Kant, should at once have been the author of the "Critique;" have anticipated, by about forty years, Laplace's nebular hypothesis of the origin of the solar system; and have propounded the most advanced theory of his century and of many years thereafter concerning the origin and diversification of human races. The eighteenth century produced the ingredients which the nineteenth developed and worked over in its efforts to account for the races of man: the significance of heredity, of environment, of geography; the biological foundations of human culture; the anatomy and physiology of man, and their position with respect to homologous phenomena in animal life in general; diversification of races; hybridization; statistics, and population pressure. We can say that it sowed the seeds of evolutionary doctrine, in the sense that some of its thinkers broke away from the conceit of immutable creations and came to surmise transformation of bodily forms. It is relevant to our story to cite further that Sanskrit was discovered and the basis of linguistics laid; that a scientific Biblical criticism began, which was destined to share in destroying the authority of Genesis in matters now acknowledged to belong to the domain of science.

The two men who respectively personify the two traditions from which systematic raciology has started are, Blumenbach and Kant. The former approaches the problem of race from the point of view of biological empiricism; the latter, as I have remarked, is the greatest race-theorist of his time. Blumenbach's approach to raciology was to become the pattern for many decades that followed; Kant's attempts were to be almost forgotten.

Immanuel Kant

It is curious to discover that certain leading anthropologists in this country have not been aware that Immanuel Kant was a "founding father" of raciology, although his influence is hard to appraise. He lectured regularly on geography and raciology at the University of Koenigsberg. His works on race are few and not bulky. They begin in 1775 and are scattered over the rest of his life.[5]

[5] See especially his *"On the various races of man"* (1775) and *"Definition of the concept of a human race"* (1785).

To Kant, race is a hereditary matter, while heredity is inherent in the organism itself and not dependent upon environment. However, a proper environmental stimulation may bring out certain inherent, though not previously expressed, characteristics. The original "undifferentiated" human stock, according to Kant, is the brunette-white. The individuals of the stock contain latent "germs" (*Keime*) and when some of the stock moves into a particular climate, the "germs" that best fit it for that environment are called into expression, resulting in morphological and physiological modifications. But this process is irreversible; for the evocation of one germ brings about the permanent suppression of a homologue that would have responded to a different environment. Therefore, even if a tribe belonging to a certain race migrates, as some have done, it cannot reshape its typology. Moreover, some people show that they have gone farther toward complete race expression, while others are less "differentiated."

Four typical races are so derived:

1. The very blond race of the North European damp-cold climate.
2. The copper-red race of the American dry-cold climate.
3. The black race of the Senegambian damp-heat.
4. The olive-yellow race of India's dry-heat.

These elemental races have produced all other races by the process of hybridization. This somewhat simplifies Kant's race theory, but it does it no essential injustice.

This is the first serious race theory on record. The diagrammatic arrangement of climates is perhaps its most obvious defect. How or why Kant arrived at such a system would lead us too far afield. Suffice it to say that it attempts to account for the mutual relationships between heredity and environment consistently with the rest of his hypothecation.

Frequently, the failures of the eighteenth-century mind to go just one step further strike the twentieth-century mind as odd, even baffling. It is, of course, a matter of the superiority of retrospect. Kant's diagram of climates did not prevent him from recognizing that climates can change. He adduces into the midst of his race studies very sharp physiographic reasons for believing that once Central Asia was a great inland sea.

We cannot help recognizing that here are seminal ideas: the evolution of races conceived as radial adaptations; their irreversible and

hereditary nature; their inherent independence of environment; the non-hereditary and purely adaptive nature of such characters as the deepened tan caused in white men by a tropical sun, in spite of the failure of negroes to blanch in Europe.

Kant is already identified, then, as one of the school which claims the original unity of the human genus. The significance of this grows when we examine the conflict between those who adhere to this view and those who believe in a multiple origin for man.

An interesting and curious feature is that Kant accepted genus and race, but not species. We cannot go into the reasons for this, but it does point out the fact that the species-concept was ill-defined in Kant's day. Neither Linneus nor Buffon had settled the matter. We should note, also, that Kant universalizes his biological laws to cover other animals. Certainly, this will be recognized as being consistently Kantian.

Johann Friedrich Blumenbach

Blumenbach's claims to the niche in which a grateful posterity of anthropologists has placed him rests upon the comprehensive scale on which he conceived physical anthropology. Man's anatomy and physiology; his resemblances to other animals and his distinctive characteristics; his special psychology; the diversities in all these matters, among men the world over; his races and their location; all these matters, he treated with the care and caution of a thoroughbred scientist. His materials were an imposing array of dissections, fetuses, crania, and hair specimens. In fact, his private "museum" was one of the show-places of Göttingen. His treatise, *"On the Variety of Mankind"* (1st ed., 1776), was to set the pattern for decades after his time, and it is still a joy to read. He is remembered today, particularly, for his interest in the shape of the skull as a race diagnostic, and his system of five human races, located in terms of five world-regions. Many a layman, today, unconsciously memorializes Blumenbach by his impression that all mankind is divided into five parts: Caucasian, Mongolian, Negro, Malayan, American Indian.

Blumenbach assumes that species are constant. Also, we must enter to his account the line that is drawn between race and species in such a way that the origin of the former can be accounted for, zoologically, while the origin of species has to be hypothecated only. This is

a weakness that will pervade both camps in the coming argument between the monogenists and the polygenists. Says Blumenbach, "We say that animals belong to one and the same species, if they agree so well in form and constitution, that those things in which they differ may have arisen from degeneration. We say that those, on the other hand, are of different species, whose essential differences are such as cannot be explained by the known sources of degeneration:"* However, as for what constitutes a specific trait, he is avowedly uncertain.

Since Blumenbach ascribes to degeneration the differences in races, we can profitably summarize its manner of occurring: "There is no motion in the animal machine without a preliminary stimulus and a consequent reaction. . . . The genital liquid is only the shapeless material of organic bodies, composed of the innate matter of the inorganic kingdom, but differing in the force it shows . . . by which its first business is under certain circumstances of maturation, mixture, place, etc. to put on the form destined and determined by them. . . . Let me be allowed to distinguish this energy, so as to prevent its being confused with the other kinds of vital force, . . . by the name of formative (*nisus formativus*); by which name I wish to designate not so much the cause as some kind of perpetual and invariably consistent effect . . . just in the same way as we use the name of attraction or gravity to denote certain forces, the *causes* of which however still remain hid. . . . As then other vital forces, when they are excited by their appointed and proper stimuli, become active and ready for action, so also the formative force is excited by the stimuli which belong to it, that is, by the kindling of heat in the egg during the process of incubation. But as the other vital forces, as contractility, irritability, etc., put themselves out only by the mode of motion, this, on the other hand, of which we are talking, manifests itself by increase, and by giving a determinate form to matter. . . . Now the way in which the formative force may sometimes turn aside from its determined direction and plan is principally in three forms. First, by the production of monsters; then by hybrid generation through the mixture of the genital liquid of different species; finally, by degeneration into varieties." Blumenbach then assigns to the various agencies in the environment the warping of the *nisus formativus*. Of course, he has noticed that negroes remain black, even if they have lived in Europe, so that he has to qualify his theory with supplementary assumptions.

* *De Generis Humani Varietate*, ed. 1795, par. 23. Bendyshe's translation.

He is so impressed with the preeminent beauty of the "Caucasian" race, particularly with a female skull from the actual Caucasus region, that he has inflicted upon us poor whites a name that we cannot, apparently, shake off. So the region just north of the Caucasus must be the most probable home for the origin of the human genus, and all other races have degenerated more or less from a pristine ideal.

I offer these sketches of Kant and Blumenbach as samples of eighteenth century anthropological thought, trusting that we all realize that it does not do justice to the richness as well as the limitations of the period. For, the period that is to follow depends, of course, largely on the richness and the limitations which are its legacy.

III

The fall of Napoleon was, in a way, a blow to science. For, hitherto, France had been the leader, and now she had many wounds to lick, some of them of many years' standing. There was no other country ready to step into her shoes. Germany was preparing to do so, but she still had a long way to go. The energies of England had long faced the seven seas quite as much as the continent to her east. Her interests were divided, and there is a limit to what a nation can do at any time.

There was another reason why at least some of the sciences languished. After the treaty of Vienna, political Europe went violently reactionary. The *status quo ante* was to be restored as far as possible. It was an age of disappointment and frustration to those who had expected a new world. Today, we know that the reactionaries were forbidding the tide. New forces were gathering and thrusting upward. Industrialism was gaining momentum. Inventions were mounting. Population was multiplying. Cities were swelling and spilling over. Socialism was born. Like the sun that shineth upon the just and the unjust alike, the British constitution was sheltering Karl Marx, Jeremy Bentham, John Stuart Mill, Thomas Carlyle. The frustrations and the *Sturm und Drang*, and also the *Aufschwung* ("upswing," "soaring") of the young men produced the romantic movements, with their nostalgia for an idealization: Scott, Byron, De Musset, Heine, the Schlegels, Pushkin, Lermontoff, Chopin, Schubert, Schumann, the Hegelians. They also produced the revolts of 1830 and 1848. Through it all, nationalism grew into something unprecedented in the world's history. European man became gradually aware of *movement*—movement

throughout the entire universe. The idea of organic evolution could not have come at an earlier period than it did.

The period saw the estrangement between philosophy and science —a development that has been expensive to both. But that tale belongs to the general history of nineteenth-century thought. It cannot be rehearsed here. It is sufficient to mention that the romantic philosophers, led by Hegel, did not speak the language of the now thoroughly empirical scientists. In a way, we might say that the school of "natural-philosophers" bridged the gap, for among these are some truly great names, such as Alexander von Humboldt, Lorenz Oken, Carl Agardh, Erasmus Darwin, Schelling, and Agassiz. Undoubtedly, we should understand the impulses in raciology better if we were to linger over them. It must suffice for our purposes that an Immanuel Kant becomes an impossibility; and that, when philosophy and science become estranged, the times are truly out of joint.

If we look for the *Zeitgeist* in science, I believe we can cover it with the terms, *movement* and *atomism*. They are obverse and reverse. They seem to be responsible for the mechanistic attitude in science, the so-called scientific materialism. At once, we must distinguish it from philosophic materialism. In science, it is merely the insistence that any scientific account must deal only with processes that are amenable to physical verification. This is why a scientist must be suspicious of any so-called fact that cannot even be conceived as being at least theoretically measurable. This attitude does not take full possession of science until the nineteenth century.

This materialism could not start with the study of man. It had to begin at the other end of the scientific spectrum and work over to man. It illustrates what I have called a cultural lag in science. The early nineteenth century discovers the true nature of heat, formulates the laws of thermodynamics, conceptualizes the conservation of energy. It is but a step to measurements of work and heat output in animals, including man. This is, to be sure, only a short step from the eighteenth century. It would not, of itself, eliminate the vitalism of the physiologist, Bichat (1802). Organic chemicals are still *organic,* until, in 1828, Wöhler synthesizes urea artificially, and the wall between organic and inorganic chemistry begins to break down. There follows the elaboration of the carbon cycle in nature, the discovery of the function of oxygen in the blood and the tissues.

This affects our particular province in two ways. Men become habituated to explaining phenomena exclusively in natural terms. Even phenomena that might be supposed extra-natural turn out to be amenable to natural treatment; as, for instance, when Gall comprehends the significance of the gray matter in the brain. (As Gall also believed in the importance of heredity, he was accused by the Church of being a "materialist" and a danger to the doctrine of free will.) Race psychology travels along with race morphology, through Blumenbach, Prichard, Broca, down to our own post-Darwinian days. Even Linneus opened a sluice when he classified races as having consistently their several particular temperaments.

However, let us not make the mistake of imagining that the eighteenth century could not think at all of a living thing—including man—in mechanical terms. It amused itself by constructing marvelous clockwork dolls. It was aware that the human body is full of levers and pulleys. Nevertheless, this same century produced deism and vitalism, and it was quite consistent in doing so. Interestingly enough, comparisons between man and ape did not shock the eighteenth century as they did the nineteenth. It is the measure between the presence of materialism in the later science and its absence in the earlier. Nineteenth-century (post-Darwinian) ape-man comparisons stood for a blood-relationship between the "soul-less" ape and man. Eighteenth-century comparisons demonstrated certain consistencies of idea in a Deistic universe. The eighteenth century set things in orderly array, thus demonstrating the rationalism of God. As mentioned before, we should understand Linneus, the naturalist, and Leibniz, the philosopher, in this way. It was possible even to construct pseudo-evolutionary theories. For instance, the bird is the *perfection* out of an ancestral flying-fish. Man, himself, can be conceived as having attained perfection out of less perfect ancestors. Kant suggests that the arrangement of man's internal organs indicates that his ancestors once went about on all fours; but, to the eighteenth century, none of this means an organic, evolutionary blood-relationship between man and ape. The ancestral tiger was less perfect than the contemporary tiger; but the whole series of generations started from a separate creation. This is De Maillet's view; it will serve as a sample.*

* Cf. Radl: *Geschichte der Biologischen Theorien*, Vol. II (1909).

This sort of thing was bound, eventually, to undermine the belief in an originally perfect creation; but, in a century of Deism, it had no trouble getting along.

The significance of *movement* to the developing racial thought will come out of itself. But *atomism* requires special attention.

The nineteenth century developed the belief that, if it could take all things apart and reach the ultimate unit, it would have explained the physical universe. It succeeded in discovering the atom and the electron. By 1830, Schleiden and Schwann had put their fingers on the real nature of the living cell (biological atomism). Animals, including man, are dissected, not merely anatomically, but in a temporal sense. That is, they are analyzed back into the embryo, and the embryo leads back to the cell; in fact, to a zygote. We should note, in passing, that movement in developmental anatomy (in ontogeny) is to become a powerful ally of movement in evolutionary anatomy (phylogeny); and so, eventually, to revolutionize the concept of race.

The eighteenth century had gone far in placing man within the framework of natural law, along with all other living things, and the momentum carries over into the nineteenth century. From this standpoint, let us understand the contribution of the Belgian astronomer Quêtelet (1796–1874) to physical anthropology and raciology. In 1835, he publishes a study, *"On Man and the Development of His Faculties."* It turns out that the same kind of frequency-curves that apply to variations in other natural phenomena are valid for man, too. Now, the measurement of man is known even earlier than the eighteenth century; but Quêtelet's work appears in the midst of the materialist trend in science. Furthermore, he measures the anatomical and physiological differences between races, *e.g.*, between Whites and Indians; and still further, he measures living people. He amasses a series of measurements and calculates their average. Out of averages, he conceptualizes the ideal man ('ideal' in a philosophical sense), the type that characterizes the tendency of the *group*, though no single individual may fit it. Both the manipulation of series and the treatment of measurements mean that man is being introduced to quantitative analysis. The individual turns out to be a law-abiding variant of a law-abiding group, just like many other natural phenomena. In 1842, Anders Retzius, the Swedish anatomist, carries Blumenbach's love of craniology to the

calipers. He measures the length and breadth of crania and takes their ratio. Now, long-heads and short-heads had been noticed long before, and even their occurrence in some systematic way as far back as Vesalius. Certainly, their ethnic significance was stated by Edwards, in 1829, as we shall note again later. But Retzius *measures,* and out of his *cranial index* he determines that all mankind falls into categories of dolichocephals or brachycephals (his own terms), and their incidence is significant geographically. By adding prognathism and orthognathism he achieves a four-way classification.

It is too bad that time will not allow us to follow out all the consequences of this radical step in metric analysis of human races. We must be content with a brief series of comments. First, head-shape could not be referred to any system of environment. It fitted no conceivable set of external circumstances, and apparently it was constant from parent to offspring. This added to the evidential strength of heredity in race. Second, the four-class scheme naturally predetermined the number of human types found. This does not lead necessarily to certain misapplications to race classification; but, as a matter of fact, it has done so, during the twentieth century, when the world distribution of such gross skull-proportions have been misidentified with actual race. Third, a dividing-line between dolichocephaly and brachycephaly is bound to be arbitrary. In fact, this has eventually been realized, and the mesocephals have been created as a buffer. This is one of several refinements that have been introduced into the system. Fourth, it reduces a solid to a plane. I cannot help wondering whether this weakness may not have delayed recognizing the difference, for instance, between Dinarics and Alpines. Fifth, Retzius then classifies mankind in terms of tribes and linguistic stocks, according to their placement in his four classes. About this confusing introduction of tribes and linguistic stocks into a physical classification, there will be more to say later. For the moment, it illustrates a very common pitfall of the period. Nevertheless, it is the fundamental principle of Retzius' method that, in later years, has been applied fruitfully to unraveling the tangle.

After Retzius, especially after Darwin laid stress on many small and "random" variations, craniometrists multiplied measurements and indices, until, at the close of the century, von Török was taking hundreds of thousands on a single skull, in an effort to reach the ultimate in

differences, and craniometry threatened to collapse of overweight. This was nothing else than the *reductio ad absurdum* of the atomistic logic as applied to human quantitative analysis. In all fairness to von Török, he himself recognized this before the whole movement was in ruins.

All these processes are shaping man to fit into a universe of natural, immutable, but mobile law. The climax is to come when evolution is introduced into the whole scheme. But much still remains to be accomplished and we must turn to some contributions from other sciences.

We have observed how Blumenbach illustrates the temper of the eighteenth century in anchoring the several races to geographic locales. In the nineteenth century, as historic depth becomes gradually applied even to an emergent prehistory, the movements of races take on an importance but poorly understood, in the eighteenth. Nevertheless, geography *is* a factor in the race scheme. It needs merely to be evaluated properly. Kant had noticed that the current of human migration set in the direction from Asia to America, that America was an ethnic recipient and not a donor. But the protean situation in the matter of human origins and race diversification that obtains in the period we are considering is reflected in Louis Agassiz. In 1845, he is a monogenist—a believer in a single origin for man. In 1850, he finds eleven or twelve distinct human species. In 1853, he has reduced them to eight, and fits them to eight faunal-floral provinces. Finally, he believes in a multiple origin for man. It is, of course, the inalienable right of any thinking man to change his mind. However, states of mind are not our principal concern. Agassiz was to become a confirmed opponent of Darwin; yet his contribution to raciology comes from studies in the very field that made an evolutionist of Alfred Wallace. Faunal provinces are a *bona fide* part of the equipment of modern zoology. What Agassiz did not realize was that, anti-evolutionist that he was, he yet identified man with the provinces, as though he were but another animal. He was impressed, apparently, with the characteristic adaptations of several masses of humans to several different environments. He was writing in a day before Edward Tyler, when Lyell was establishing historical geology along modern lines, but had not come to be accepted universally; when the *evolution of faunal-floral provinces* was no more than a germ in the minds of such as Wallace; when great, secular movements of man, reaching back even into

sub-human ages, and occurring while climate, too, evolved, were still a concept unseizable by the scientific imagination. However, we cannot, at this time, go into the great subject of anthropogeography and its significance to raciology. The subject was very much alive in the late nineteenth and in the twentieth century.

We have now surveyed contributions from a number of sciences to a gestating anthropology. It is high time that we take up again that strand which after all must ever remain central to scientific raciology: the comparative anatomy and physiology of man—the tradition of Blumenbach, of Cuvier, Geoffroy St. Hilaire, Darwin, Lawrence. The early nineteenth century teems with names that deserve more than mere mention. The leadership of France still shines forth in the preponderance of Frenchmen. But we can catch the flavor of the age by limiting ourselves to Georges Cuvier.

Georges Cuvier

Tradition credits Cuvier with the tripartite division of mankind into blacks, whites, and yellows. Actually, tripartite division was also known to the eighteenth century. Furthermore, Cuvier lists a residue of Malays, Papuans, and American Indians that do not fit his tripartite scheme and, therefore, are of uncertain position. It is very much like accepting three of Blumenbach's categories and doubting the other two. But even if Cuvier did not originate the classification, it is very certain that the enormously painstaking founder of comparative anatomy and paleontology had far more factual evidence to back his taxonomy than his predecessors had. Cuvier's prestige dominated the first third of the century. The tripartite scheme passes down through Huxley (but suitably modified) and is, I believe, the most widely-accepted race scheme among contemporary anthropologists. What seems to me most impressive, however, in this first half of the nineteenth century, is the failure of zoological principles to progress in raciology. More empirical, biological facts about man are gathered in, but new insight into race does not seem to come from them. That is, Blumenbach sounds hardly less modern, a generation after his death, than he did when he was alive. When we come to treat of the use of linguistics in the contemporaneous classifications of man, we shall realize that the conservative caution of empirical biology prevented it from meeting a need which was felt by raciologists.

Let us return to Cuvier, who was far more significant than as the propounder of a usable tripartite taxonomic scheme.

He is remembered, of course, for his famous debate with Geoffroy, in 1830, in which he ostensibly defeated the latter, who stood for some kind of evolution in nature. Cuvier, therefore, is remembered as an opponent of evolution. To Cuvier, the fossils seemed to say that there had been several epochs of past life on earth, each destroyed by some great natural catastrophe; whereupon, the earth was repopulated from living things that had escaped destruction in some asylum. Cuvier's followers turned these repopulations into successive, independent creations, and the perversion has been unjustly ascribed to Cuvier, who has explicitly denied this to be his view. As to where each new seeding came from, he was agnostic. He did believe that each successive inter-catastrophal epoch shows a higher system of fauna. Man did not appear until the present epoch; where from, Cuvier did not say. He declared categorically, "There are no human fossils." For his day, he was right. Cuvier himself had examined alleged human fossils and had found them erroneous—some of them ridiculously so.*

Cuvier, however, rendered a service that cannot be overestimated. While his catastrophism was a mistaken interpretation, soon to be refuted by the classic geological explorations of Sir Charles Lyell, it was Cuvier who gathered and arranged the empirical evidence that there had been a progressive succession of faunas on earth. As a result, the static picture, which was all that the eighteenth century could offer, begins its slow movement. With Cuvier, biological nature begins to turn kinetic. The evolution of human races is involved along with all other evolution.

EDWARD LAWRENCE

This greatest physical anthropologist, between the time of Kant and Darwin, cut an unhappy figure. At 34 (in 1817), the famous surgeon and professor made utterances not heard again for over two generations. He raised a tremendous hue and cry. His published lectures

* The Cannstadt calvarium was found in 1700. It was not pictured or described until 1835, and then, but poorly. We do well to remember that a Gibraltar specimen, discovered in 1848, and the first Neanderthal specimen, found in 1856, were all the paleontological evidence of ancient man in existence before the publication of Darwin's "Origin of Species." One has no right to expect a scientist to pin his convictions to as slender a piece of incipient and unprecedented evidence as the Gibraltar specimen. Schaaffhausen described the Neanderthal skull. As he had already announced his belief in evolution before Darwin published, he boldly welcomed this corroboration. Virchow, the pathologist, declared the specimen to be merely pathological *Homo sapiens*. In the state of knowledge in that day, the interpretation was quite reasonable.

were refused copyright. He was forced by his superiors to repudiate them; and, although he is known to have had the natural courage of his convictions, for some reason never again did he busy himself with anthropology. What his maturer mind might have produced is an attractive speculation. He lived to advise Darwin against publishing his ideas, lest he be pilloried; but the ideas of the young man in his thirties continued their momentum underground, as repeated and widespread references to them during this period testify. Because he could not procure a copyright, he was forced to see publishers pirate edition after edition of his lectures, to which he never contributed further.

Lawrence dedicated his lectures to Blumenbach. From the latter, obviously, he derived his comprehensive scope for surveying mankind. Although he found Blumenbach's race classification wanting in several respects, he did not undertake to improve upon it, but showed himself to be a keener biologist than Blumenbach. His lectures are still an inspiration to read. He says:*

"1. The differences of physical organization and of moral and intellectual qualities, which characterize the several races of our species, are analogous in kind and degree to those which distinguish the breeds of the domestic animals; and must, therefore, be accounted for on the same principles.

"2dly, they are first produced, in both instances, as native or congenital varieties; and then transmitted to the offspring in hereditary succession.

"3rdly, of the circumstances which favor this disposition to the production of varieties in the animal kingdom, the most powerful is the state of domestication.

"4thly, external or adventitious causes, such as climate, situation, food, way of life, have considerable effect in altering the constitution of man and animals; but this effect, as well as that of art or accident, is confined to the individual, not being transmitted by generation, and therefore not affecting the race."

The following statement concerning what is hereditary and what is not is even startlingly modern:

"When the fetus in utero has small-pox or syphilis, there is actual communication of disease by the fluids of the mother. This is a case

* *Lectures on Comparative Anatomy, Physiology, Zoology, and the Natural History of Man.* 470 ff. 1823.

altogether different from those under consideration. Neither does hereditary predisposition to particular diseases prove that acquired conditions are transmitted to the offspring. There are natural varieties of organization, disposing different individuals to different diseases on application of the same external causes. The natural varieties, like those of form, colour, and other obvious properties, are continued to the children."

That the influence of Lawrence, like that of Kant, should have been largely lost from raciology, is a great pity. That Lawrence can hardly have been "*spurlos versenkt,*" however, is suggested by the pirating of his book. Topinard related that Broca made him read Lawrence's lectures, and Cunningham, in 1908, who repeated this remark, said that it was his practice, too, in connection with his own students in anthropology.

We have been feeling the temper of scientific thought in this earlier half of the nineteenth century, and will soon be turning to the other great influence upon raciology. We cannot do better, in winding up this part of our discussion, than treat of the prime controversy over race and species that stormed down the decades and continues even in our own day. It makes a better transition, because it was effected by a vexed social and economic problem of the day, and it demonstrates that even scientists are human.

I refer to the battle between the monogenists and polygenists, in which the contestants still carry the aroma of Biblical tradition in their vestments, and where the negro slave suffers the usual fate of the innocent bystander.

Again, we must go back into the preceding century, and recall a remark by our contemporary, Toynbee, that our earlier efforts towards arriving at an explanation of man's races utilized the theological materials already at hand. Can Adam and Eve assume the responsibility for kinds of men obviously unmentioned in the Bible? In 1655, la Peyrère did a courageous thing. He published his *Pre-Adamites*—and became the progenitor of the polygenists, who have always believed that humankind has a multiple origin.

Now, if you are a polygenist, you must explain the interfertility of the numerous human "species" (races). If, on the other hand, you believe that all mankind descended from one species, in spite of every objection, you must account for the diversity of races and evaluate

heredity and environment in producing so much diversity from a slender original stock. It really is a difference in your psychology. As a polygenist, you are impressed with the diversities. As a monogenist, you prefer similarities. Which are the more fundamental? On what premises do you answer that question? Your school, moreover, will affect your receptivity towards the very meager evidence as to human hybrid fertility and quality.

Today, we can see that the tide was setting against the polygenists, long before it was realized by the warring parties themselves. While the polygenists included some doughty champions, such as Agassiz, Pouchet, Nott, and Glidden, the most memorable names in the history of raciology are monogenists; *e.g.*, Blumenbach, Cuvier, Lawrence, Prichard. Blumenbach derived the colored races from the white by degeneration. Cuvier seems to have been aided to his tripartite scheme by Noah's three sons (as others have been). Prichard thought original man was black and that the rest of us have undergone various intensities of bleaching. In any case, it is no great mental gymnastics to imagine that men have endlessly "degenerated" or diversified, however they have done it. You find mankind as full of races as you wish; which, in fact, was what race-taxonomists were doing, under a variety of assumptions. But, if you multiply original "species" under the polygenist assumption, you call into being a new set of stem-parents with every new identification, unless you account for some of them by hybridization; in which case again, you are confronted with an endless interspecific fertility, and to the polygenists this idea often was not congenial.

An attempt to outline in a very few sentences as diversified a controversy as this one, and also one in which the hypotheses have received the stamp of time and have been relegated to the archives, is always likely to be unjust to the caliber of the disputants. Perhaps the thought-fashion of the days just before Darwin can best be hinted at by a short quotation from the judicious Waitz, who writes on the eve of the *Origin of Species:**

"In condensing the results of our investigation regarding the definition of species, we have found that it designates those types permanent which are transmitted by propagation. We were induced to separate the questions of unity of species and unity of descent on the ground

* Waitz *Anthropology*, I. Collingwood's translation: 32 ff. 1863.

that the same assemblage of constant characters may belong originally to distinct stocks; and we could not, therefore, consider unity of descent as necessary to our definition of species. If, thus, separate descent was no valid proof for difference of species, unlimited prolificacy proved an important, but not a decisive mark of distinction between species and race, and could only be considered as a probable sequence.

"Finally, reversion and its allied phenomena appeared insufficient to furnish an undoubted criterion of species and race. And as it became apparent that such a criterion could not be established, we hoped that the defect would be supplied by the conclusions of analogy furnished by the comparison of various species. This expectation was also doomed to disappointment, as the limits of variation in different types seem to be of a greatly diversified extent. . . . The general question as regards a decided mark of distinction between race and species can only be answered by the particular study of the extant variation in individual types. . . .

"The investigation of the unity of mankind as a species can only be finally completed, when the results of long continued influences of all possible external conditions in which man is able to live, are as fully and clearly ascertained, as the results of all possible crossings of various human types after a long series of generations. But as our experience in this respect is very far from being perfect, we are compelled to stop at some more or less probable propositions, which must proceed from the solution of the question, whether a gradual alteration of types belonging to the same stock can be proved, and whether it be sufficiently extensive in order to show that the greatest differences prevailing among mankind are merely variations. Next to that the question will be, whether the cross-breeds of the various types, by limited prolificacy, or by constant reversion to the parent type, resemble more the hybrids or mongrels of different races."

It seems that the futile impasse to which argument between the camps had come was due to their failure to absorb the mechanistic temper of the age enough to think of species and race in the same way. They seemed to place the two concepts in different compartments of thinking, at least at the start of the controversy. About species, they did not postulate the mode of origin. They accepted species as a starting-point, whatever a species may be, and then sought to derive races by quite natural means. At least, that was the essential method of

the monogenists. But unless we appreciate the practice of assuming the fact of species and then focusing upon race origins, we cannot sense the explosive impact of such an eventful title as the *Origin of Species*. Even the great geologist, Sir Charles Lyell, who founded historical geology and overthrew Cuvier's system of successive catastrophes, was long reluctant to accept Darwin's hypothesis, much to Darwin's chagrin and disappointment.

It is interesting that both monogenists and polygenists were convinced of the superiority of the white race; although, to be fair, there were some who suspended judgment about racial "inequality." The humane Lawrence stated that if the negro is inferior, nevertheless, he is human. This is an echo of Soemmerring (1785). "But if the white is superior," says Lawrence, "then *noblesse oblige*." Lawrence spoke in the days when England was getting rid of the slave trade. There were pro-slavery people who accepted monogenism and cited Noah's curse of Ham to justify themselves. On the other hand, polygenism seems to have been a more congenial attitude, if you were a pro-slaver. It was favored by many (see Nott and Glidden). That the whole question of the position and status of the negro agitated the students of man, is witnessed by a very long deliberation by the French society of ethnology. The role of science is to ascertain the truth, and not to engage in social propaganda. The deliberation was to ascertain the truth. It demonstrated perfectly how society posed a problem which the students of man were obliged to consider. At the same time, as far as I know, none of the conclusions of the scientists had any effect whatsoever in settling the issue of negro slavery.

Since our story is to go no farther than the eve of Darwinism, let me clinch the difference of outlook in biology before and after this event, for raciology eventually partakes of it.

"We have had occasion," says Radl,* to point out that the new natural science, instead of seeking to *comprehend* nature, aspired to an *intellectual reproduction of natural events;* that instead of *concepts* of nature it sought a *photograph* of nature. Everywhere we see how, in keeping with this endeavor, the earlier *concepts-in-common* were banished from the biological sciences and were replaced with *things-in-common*. Whereas the old natural-scientist spoke of the ideal vertebrate type, the new one speaks of a real ancestor of the vertebrate,

* *Op. cit.* II: 378 ff.

which occurred somewhere and sometime; formerly, one reflected about metamorphoses and unity of plan, now one discusses transformations which occurred at a certain time and proceeded from a common origin; the former *ideal* similarity is now replaced by a *material blood-relationship.*"

IV.

We have been tracing, thus far, the biological tradition from which the idea of race in man has derived. We have remarked that anthropology does not become a self-contained and unified discipline until the mid-century. We have seen this evidenced by the fact that the most important ideas that have gone into it were wrought out in other disciplines and then applied to man; while that region which is peculiar to raciology, namely, the classification of races, is the least progressive part of the whole concept, because its biology, after all, is not deep enough. We have noted the divorce between philosophy and science. In this period, there comes to raciology a new mass of influence; and it is not from the laboratory or the explorer's journal, but from the clash and clang of the life of nations. I have no better name for it than the cultural or the social as contrasted with the biological influence. It has to do with that other great adjustment which European man has been making: the adjustment within his own bounds, the understanding of himself down to the very biological roots which his fast-developing nationalism has demanded of him. It has to do with that use of the word "race" on a smaller scale, to distinguish the various strains of which he is composed. He will try to unravel these strains and trace whence they come. History has already told him that some of them, at least, are invaders within cultural memory. But what of the others? Perhaps language can help identify them. In the absence of written documents, the evidence of the new science of prehistoric archeology must serve. In last analysis, to serve for identification, there can be no substitute for the actual corporal man. The real significance of heredity compared with environment must be settled, and so the road leads back to biology.

In the eighteenth century, Europe discovered Sanskrit; and, thereafter, that most of the languages lying in a swathe from Iceland to India belong to a common family and indicate a common original. It was one more earnest of universal evolution that European thought was building up before Darwin. It came at a time when European

nationalism was beginning to soar; at a time when the *people* of a nation were taking the stage of attention more pervadingly and in a more sophisticated manner than ever before. It is as though the nations were following Napoleon's military *levée en masse* with a cultural *levée en masse*. . . .

In 1828, in the recently-defeated France, Amédée Thierry published his *History of the Gauls*. The last word is significant. It bespeaks a progression since the day when Gibbon wrote *The Decline and Fall of the Roman Empire*. In 1829, W. F. Edwards published in antiphony to Thierry *The Physiological Characteristics of the Human Races*. Institutions and their history may pass away, but peoples endure. Edwards concentrated on the people of France, but passed far beyond her borders. There are two distinguishable types in France, that occur also in neighboring countries. One is medium in stature, rather dark, and round-headed including the "Gauls," whose speech was once Celtic. The other is a tall and long-headed race, the "Kimri," alias the "Belgae." Both natural history and "civil" history, according to Edwards, testify that they have maintained constancy since antiquity; which means that, in this case, heredity is stable. Edwards cited the experiments of Coladon on the crossings of black and grey mice. The two strains appear separately in the offspring. He applied this to man. "The human races that differ the most from each other," he says, "constantly produce hybrids. Thus a mulatto is always the result of a mixture of white and black races. The other observation, that the two original types are produced when the parents belong to two close varieties, is less notable, but it is none the less true. The fact is common among the European nations. Crossing produces sometimes fusion, sometimes separation of types."

The French had long been used to the knowledge that they were of mixed origin. Edwards initiated the investigation that was later carried to a brilliant point by Broca (1860), which demonstrated the racial make-up of the nation and related it to the historical and linguistic background. The significant point, here, is that Edwards started a line of investigation that determined the relationships—and non-relationships—between race, language, and nation, on a historical and anthropological basis. The fact of race diversity in Europe transects national boundaries at the very time when nationalism is becoming more intensively significant, and the fact owes its elaboration, in part, to the national self-consciousness with which it does not coincide.

The linguists, meanwhile, were busy with the question, "What was the original Indo-European tongue?" To which, the students of race respond, "Yes, but who were the Indo-Europeans?" It was natural to reason that so distinctive an entity as an Indo-European language must once have been carried by a distinctive strain of people. By now, no raciologist would argue that climate makes the difference between a Swede and a Hindu. However, language eventually proved to be a less reliable and fruitful guide to race identification than it had seemed to promise. Nevertheless, who were the people who originated Indo-European speech in Europe?

James C. Prichard

In this period, the greatest figure is the English Quaker physician, Prichard, whose monument is his *Natural History of Man* (1843) and his *Researches into the Physical History of Man* (1826–1847, and later). The first volume of the latter work starts with fauna and flora, describes man the animal, and then takes up the problem of race in man. However, this encyclopedic and very painstaking research then follows *tribes* back into their history, and describes what, today, would be ethnology. Nevertheless, it does not define "race." Prichard's linguistic division of Europe and Asia is into Indo-Europeans and "Allophylans."

Now, Europe rushed to a climax.

In 1853, Gobineau published his *Essay on the Inequality of Races;* embarked on a series of wars: against Austria (with Sardinia), against Russia (with England), against Mexico, and finally against Prussia and her German coalition (1870–1).

In 1853, Gobineau published his *Essay on the Inequality of Races;* dedicated it to the English crown; was ignored by his own compatriots and, eventually, was taken up by their rivals across the Rhine;

1856, Quatrefages became the first Professor of Anthropology;

1858, The English crown took over the government of India;

1858, Broca founded the first Anthropological Society;

1859, Darwin's *Origin of Species* was published;

1860, Sardinia became the Kingdom of Italy;

1860, Broca's Researches into the Ethnology of France appeared;

1861, Nationhood in the United States was brought to its critical test;

1863, The Anthropological Society of London was founded;

1866–1871, Prussia eliminated the challenges of Austria and France for the hegemony in Europe, and founded the German Empire; and in 1871, Italy took Rome;

1865, Anthropological or Ethnological Societies were founded in New York, St. Petersburg, Moscow, and from 1866 to 1874, in Manchester, Florence, Berlin, Vienna, Stockholm, and Tiflis.

I have presented the dichotomy of tradition behind the concept of race in the pre-Darwinian nineteenth century: a biological tradition and a social one. The former, we have watched take shape out of an eighteenth-century matrix, in which the philosophical aspect, embodied in its highest form by Kant, was sloughed off; while the aspect best embodied by Blumenbach was continued. We have noted, also, that general biology underwent a certain critical development, during the period we have examined particularly, but that the application to man remained such as to warrant our speaking of the period as one of gestation only. On the other hand, the social tradition grew mightily, particularly in France; so that raciology drew sustenance from a rapidly-developing ethnology. In fact, I think we should find raciology in danger of becoming but a part of ethnology. By mid-century, ethnology and anthropology were a pair of fidgeting nestlings not quite sure of their respective rights in a common nest Also, do not imagine that the birth of the anthropological societies was a simple triumph of circumstances. The midwives who attended had their problems. Broca's anthropological society in Paris was tolerated on condition that the police attend each meeting. In England, the Anthropological Society ran into a maze of suspicions and jealousies among the scientists —the very people who should have cheered its founding. (Admittedly, the founders must share the blame therefor.) The point is, that again we have one of those many, many instances when the times are indeed ripe for a new movement, but only a small portion of society is sensitive enough to realize it.

While Napoleon III and Bismarck were duelling before the footlights, an equally significant struggle was going on upstage. I have spoken of Broca's founding the Anthropological Society of Paris and his publishing in its first Mémoire his classic *Researches into the Eth-*

nology of France; furthermore, that he so builds beyond Edwards. France seeks the sources of her greatness in the excellence of her mongrelization on a soil she is peculiarly in love with; while an emergent Germany welcomes Broca's compatriot because he points to a polarly contrasting source: the romance of either a pure-bred strain or an integrated ethnos (or both) that must be preserved intact. The German story is told to better advantage in a treatment that runs away beyond the limits we have set ourselves; and our time is running out.

Let us conclude by speaking of Gobineau.

He is celebrated for his tenet of an Aryan race, superior to all others, which brings the blessings of culture wherever it goes. There is no other race that does this. When it enters among a non-Aryan people, it degenerates by interbreeding and so disappears. Its salvation, then, rests in its native soil—remote from the Mediterranean. Gobineau obviously has drawn nourishment from the developments in ethnology and raciology in his homeland.

I would emphasize that Gobineau is not a unique phenomenon; that he stands forth under the peculiar lighting-effects of latter years, because of circumstances subsequent and entirely independent of his writings. For the belief that a superior race is endangered biologically when it passes from its homeland to any or all quarters of the globe, was very rife in the mid-nineteenth century. Back of it lay the commercial and colonizing expansions of Europe. There were Englishmen who worried about the Anglo-Saxon in the tropics, in Australia, and even in the United States. The peculiar service of Gobineau was the particular furtherance he gave to Aryanism, and this *within* the continent of Europe. Again, a very intricate and significant subject must forego analysis. I shall merely hint at it by remarking that Angles and Saxons were more substantial, historically, than "Aryans," and, while British penetration into exotic lands has repeatedly been dire, it can hardly parallel the situation of a racism that arises within the ever-tightening confines of Central Europe, with its extremely high cultural metabolism and long-standing rivalry between two nexuses that are cultural peers—Germany and France.

So let me speak briefly of Robert Knox, the British equivalent of Gobineau, an intense believer in "race," and a "Saxon" patriot. His *Races of Man: a Fragment* first appeared in 1850—before Gobineau's book. In some ways it outdoes Gobineau. Aside from that, he fears

for the degeneration of the emigrant Saxon. Nevertheless, he can declare the vices and imperfections of the "Saxon race" as virulently and as thumpingly as its virtues. He does not like to see the Saxon exposing himself disastrously in situations that bring out his weaknesses, and Saxon patriotism does not preclude an admiration of the "Celtic race" in its place. The point is, that Knox makes all the currents of history racial, at base, a tradition that has lasted to our own times.*

* Cf., e.g., R. B. Dixon's *The Racial History of Man.*

SECTION OF BIOLOGY

JANUARY 17, 18 AND 19, 1946

Conference on "*Antibiotics.*"

The Section of Biology held a Conference on "Antibiotics," as the third in the series for the Academic year, 1945–1946.

The program consisted of the following papers:

Thursday, January 17. Chairman, Selman A. Waksman, New Jersey Agricultural Experiment Station, Rutgers University, New Brunswick, New Jersey.

"Antibiotic Substances, a Contribution of the Microbiologist," by Selman A. Waksman.

"The Development of Improved Penicillin-Producing Molds," by Kenneth B. Raper, Fermentation Division, Northern Regional Laboratories, Peoria, Illinois.

"Metabolism and Penicillin-Producing Molds," by W. H. Peterson, M. J. Johnson, and R. H. Burris, University of Wisconsin, Madison, Wisconsin.

"Production of Antibiotic Substances by Basidiomycetes," by W. J. Robbins, F. Kavanagh, and Mrs. A. Hervey, Department of Botany, Columbia University and The New York Botanical Gardens, New York, N. Y.

"Production of Antibiotic Substances of Actinomycetes," by Selman A. Waksman, A. Schatz, and D. M. Reynolds, New Jersey Agricultural Experiment Station, Rutgers University, New Brunswick, New Jersey.

"Production of Antibiotic Substances by Bacteria," by J. W. Foster and H. B. Woodruff, Merck and Company, Rahway, New Jersey.

Friday, January 18. Chairman, Hans T. Clarke, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Isolation and Characterization of Penicillins," by Oskar Wintersteiner, The Squibb Institute for Medical Research, New Brunswick, New Jersey.

"The Skeletal Structure of Penicillin," by Karl Folkers, Merck and Company, Rahway, New Jersey.

"Constitutional Studies of Penicillin," by Vincent du Vigneaud, Cornell University Medical College, New York, N. Y.

"Artificially Produced Penicillins," by Ervin C. Kleiderer, Eli Lilly and Company, Indianapolis, Indiana.

Saturday, January 19. Chairman, Chester S. Keefer, Evans Memorial Hospital, Boston, Massachusetts.

"Pharmacology of Streptothricin and Streptomycin," by Hans Molitor and H. Robinson, Merck Institute for Therapeutic Research, Rahway, New Jersey.

"Pharmacology of Penicillin," by Geoffrey Rake and Arthur P. Richardson, The Squibb Institute for Medical Research, New Brunswick, New Jersey.

"Chemotherapy of Streptomycin," by H. C. Hinshaw and William H. Feldman, Mayo Clinic, Rochester, Minnesota.

"Chemotherapy of Penicillin," by Chester S. Keefer.

"Some Considerations of the Clinical Application of Streptomycin," by Garfield G. Duncan, Major A. Kleinman, and Captain E. Pulaski, Jefferson Medical College, Pennsylvania Hospital, Philadelphia, Pennsylvania, and Halloran General Hospital, Staten Island, New York, N. Y.

"Use of Antibiotics in the Navy," by Commander Werner Duemling, (MC), USNR, Bureau of Medicine and Surgery, Washington, D. C.

"Methods of Testing Antibiotic Substances and Limitations Involved," by Henry Welch, William A. Randall, and Lila A. Knudsen, Food and Drug Administration, Washington, D. C.

THE NEW YORK ACADEMY OF SCIENCES

announces

THE A. CRESSY MORRISON PRIZE CONTEST FOR 1946

The New York Academy of Sciences announces three prizes offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes I, II and III, all of which will be awarded in December, 1946. Prize I, of $500, will be awarded for the best paper on solar and stellar energy as defined below. Prizes II and III will be awarded for the best papers on a scientific subject included within the field of The New York Academy of Sciences and its affiliated Societies. The terms governing these competitions are detailed herwith.

Prize I

A prize of $500 is offered for the paper adjudged by the Council of the Academy to be the most meritorious contribution on the subject of the source of solar and stellar energy.

Introductory Statement

Understanding of the source of solar and stellar energy begins with Helmholtz's contraction theory (1854). As the primordial star contracts, the kinetic energy of the mass particles closing in under the force of gravity is transformed into heat energy. Whereas this is still believed to be cosmologically the first cause of stellar radiation, it has been realized since the end of the previous century that the process of contraction would run to its end in a time that is short as compared to the age of the earth; and, ever since this was realized, astronomers have been compelled to postulate that the Helmholtz contraction must be retarded and, for the major part of the life of the star, probably balanced by an internal pressure caused by energy that does not derive from kinetic energy. The way out of this difficulty was cleared theoretically (1905) by Einstein's law of the equivalence of mass and energy, although the exact mechanism whereby, under stellar conditions, matter would change into radiation, remained still a secret. The first laboratory transmutation of nitrogen into an isotope of oxygen by Rutherford (1917) opened the field of nuclear reactions which led to the experimental results; namely, that the mass lost in a nuclear reaction and the energy set free are in accordance with Einstein's law. By examining all possibilities of reactions that could take place under conditions prevailing in the sun, Bethe (1939) succeeded in singling out the one reaction that should, both as to the requirement of temperature and as to the availability of the elements involved, take place at the proper rate. This is the so-called carbon cycle whereby the energy liberated is equivalent to the mass defect of the helium atom as compared to four hydrogen atoms.

Prodigious progress has been made since the first A. Cressy Morrison Prize on the above subject was offered in 1926.* It is, however, felt that the complete answer has not yet been given and many questions remain open. In the first

* The subject for competition originally suggested by Mr. Morrison was as follows:

"The principle source of energy of the sun is intra-atomic energy existing within the normal atom itself which is released from the atom under the conditions of temperature and pressure which exist in the sun."

An A. Cressy Morrison Prize on the above subject was awarded in December, 1926, to Donald H. Menzel of Lick Observatory, University of California. The same author, in collaboration with P. B. Gerasimovic of Harvard College Observatory, also won the prize awarded in December, 1928. In 1930, the prize was awarded to Professor H. von Zeipel, of the University of Upsala, Sweden, for his paper entitled, *"The Evolution and Constitution of Stars"* and the most recent prize was awarded in 1938 to Hans Bethe.

place, no final model for the sun, properly satisfying the observed luminosity and the hydrodynamical considerations, has been published. Secondly, at best a start has been made on the problem of the so-called "red giants," "sub dwarfs," and "white dwarfs." Through the continued interest of Mr. Morrison and his desire to stimulate further research in the subject, the above prize will be renewed for award in 1946.

Prizes II and III

Two prizes of $200 each, offered by Mr. A. Cressy Morrison, to be known as the A. Cressy Morrison Prizes in Natural Science, will be awarded at the Annual Meeting, December, 1946, for the two most acceptable papers in a field of science covered by the Academy or an Affiliated Society.

Conditions

(1) Eligibility. The competition for Prize I is open to all. Authors and co-authors competing for prizes II and III shall be members in good standing of The New York Academy of Sciences and Affiliated Societies, but non-members may become eligible by joining one of these organizations before the closing date.

(2) Date. Papers are to be submitted on or prior to October 1, 1946, to the Executive Secretary of The New York Academy of Sciences, at The American Museum of Natural History, Central Park West at 79th Street, New York, N. Y.

(3) Papers. All papers submitted must embody the results of original research not previously published. The manuscript shall be typewritten, in English, accompanied by all necessary photographs, drawings, diagrams and tables, and shall be ready for publication. Papers must be accompanied by a summary of the data presented and conclusions reached.

(4) Awards. The awards shall be made by the Council of The New York Academy of Sciences. If, in the opinion of the judges, no paper worthy of a prize is offered, the award of a prize or prizes will be omitted for this contest.

(5) Publication. The Academy shall have first option on the publication of all papers submitted, unless especially arranged for beforehand with the authors, but such publication is not binding on the Academy.

(6) Wherever and whenever published, the papers awarded the prizes shall be accompanied by the statement: "Awarded an A. Cressy Morrison Prize in Natural Science in 1946 by The New York Academy of Sciences."

Such statement in substance must also accompany any formal publicity initiated by the author regarding the prize paper. If published elsewhere, six copies of each prize paper must be deposited shortly after publication with the office of The New York Academy of Sciences.

THE NEW YORK ACADEMY OF SCIENCES,

Central Park West at 79th Street, New York, N. Y.

EUNICE THOMAS MINER,
Executive Secretary.

NEW MEMBERS

Elected January 24, 1946

LIFE MEMBER

Tainter, Maurice L., M.D., Pharmacology and Medical Research. Director of Research, Winthrop Chemical Company, Inc., Rensselaer, N. Y.

SUSTAINING MEMBERSHIP

Naugle, John Jay, Physics and Chemistry. President, John Jay Naugle Laboratories, New York, N. Y.

Owen, Joseph Walker, M.D., Medicine (Psychiatry). New York, N. Y.

Ruskin, Simon L., M.D., Biochemistry and Medicine. New York, N. Y.

Silber, Robert H., Ph.D. Department Head, Biochemistry, Merck Institute, Rahway, N. J.

ACTIVE MEMBERSHIP

Ablondi, Frank B., B.A., Biochemistry, Physiology. Research Chemist, Lederle Laboratories, Pearl River, N. Y.

Adams, Sidney F., M.A., Geology and Mineralogy. Assistant Manager, The New Jersey Zinc Company, New York, N. Y.

Ales, Victor, M.A., Psychology, Personality and Vocational Guidance. Instructor, Psychology, School of Commerce, New York University, New York, N. Y.

Alicino, Joseph F., M.A., Microanalytical Chemistry. Squibb Institute, New Brunswick, N. J.

Altschul, Rolf, Ph.D., Organic Chemistry. Member of Faculty, Sarah Lawrence College, Bronxville, N. Y.

Anderson, George W., Ph.D. Research Chemist, American Cyanamid Company, Stamford, Conn.

Aronson, Lester R., Ph.D. Biology—Animal Behavior. Assistant Curator, Department of Animal Behavior, American Museum of Natural History, New York, N. Y.

Baer, Harold, Ph.D., Organic Chemistry. Research Associate, Bacteriology, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Bartle, Glenn G., Ph.D. Geologist, E. Holly Poe and Associates, New York, N. Y.

Berger, Julius, Ph.D., Biochemistry and Microbiology. Senior Chemist, Hoffman-LaRoche, Inc., Nutley, N. J.

Berman, Nathan, Ph.D., Nutrition and Medicine. Member of Technical Staff, U. S. Vitamin Corp., New York, N. Y.

Berman, Sidney, M.D., Muscle Physiology. Resident in Neurology and Neuropathology, Mt. Sinai Hospital, New York, N. Y.

Bernheimer, Alan W., Ph.D., Bacteriology and Immunology. Assistant Professor of Bacteriology, New York University College of Medicine, New York, N. Y.

Black, Maurice M., M.D., Neoplastic metabolism. Physician, Brooklyn, N. Y.

Bourret, G. Weston, B.S. Mining Geologist, Union Mines Development Corp., New York, N. Y.

Breitbart, David Siege, Zoology. Pre-Medical Student, College of the City of New York, New York, N. Y.

Brereton, John G., Ph.D., Food and Pharmaceutical Chemistry. Chemist in charge of Product Development, Sheffield Farms Company, New York, N. Y.

Brown, Jack Harold, B.S., Physiology and Biochemistry. Research Associate, Bureau of Biological Research, Rutgers University, New Brunswick, N. J.

Cantoni, Giulio L., M.D., Biochemistry. Assistant Professor in Pharmacology, Long Island University College of Medicine, Brooklyn, N. Y.

Capps, Elbert R., M.A. Instructor in Physiology, New York Medical College, Flower and Fifth Avenue Hospitals, New York, N. Y.

Carson, Stanley F., Ph.D., Physiology and Biochemistry. Head, Microbiology Department, Wyeth Institute of Applied Biochemistry, Philadelphia, Pa.

Carter, Anne Cohen, M.D., Endocrinology. Assistant in Medicine, New York Hospital, New York, N. Y.

Chase, Jeanne H., Ph.D., Bacteriology, Immunology. Department of Biology, Schering Corporation, Englewood, N. J.

Cieslak, Arthur K., M.D., Surgery and Surgical Pathology. Assistant in Surgery, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Clark, Joe H., Ph.D., Organic Chemistry and Chemotherapy. Research Chemist, American Cyanamid Company, Stamford, Conn.

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Craver, Bradford N., Ph.D., M.D., Pharmacology. Ciba Pharmaceutical Products, Inc., Summit, N. J.

Crittenden, Phoebe Jeannette, Ph.D., Physiology and Pharmacology. Research Associate, Merck Institute, Rahway, N. J.

Cuttita, Joseph A., D.D.S. Assistant Professor of Dentistry, Columbia University, New York, N. Y.

Dahl, Roland J., B.S., Medical Science. Director, Product Development, E. R. Squibb & Sons, New York, N. Y.

Davenport, Horace Willard, Ph.D. Professor and Head, Department of Physiology, University of Utah, School of Medicine, Salt Lake City, Utah.

Dimond, Albert Eugene, Ph.D., Plant Pathology and Physiology of Fungi. Associate in Plant Pathology, Connecticut Agricultural Experiment Station, New Haven, Conn.

Donovick, Richard, Ph.D., Bacteriology. Research Associate, Squibb Institute for Medical Research, New Brunswick, N. J.

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Drago, Anna M., B.A., Biology. Graduate Student, College of New Rochelle, N. Y.

Duca, Charles J., M.S., Medical Bacteriology. Assistant, Department of Bacteriology, Columbia University, New York, N. Y.

Dunn, Max Shaw, Ph.D. Professor of Biochemistry, University of California, Los Angeles, Calif.

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Gordon, Samuel M., Ph.D., Chemistry. Director of Research (Vice-President), Endo Products, Inc., Richmond Hills, N. Y.

Greenstein, L. M., Ph.D., Physical Chemistry. Chemist, Mearl Corporation, New York, N. Y.

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Harris, Ad, Immunology, Bacteriology, Chemistry related to Serology. Serologist, V. D. Research Laboratory, U. S. Marine Hospital, Staten Island, N. Y.

Harris, Ira Henry, Ph.D., Biochemistry, Bacteriology, Nutrition. Director, Metchnikoff Laboratories, New York, N. Y.

Harrower, Molly, Ph.D., Psychology (Clinical Application). New York, N. Y.

Hays, Edwin E., Ph.D., Biochemistry (Anemia), Assistant Professor of Biochemistry, University of Vermont, College of Medicine, Burlington, Vt.

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Hewitt, Eric John, Ph.D. Consulting Chemist, Ralph L. Evans Associates, New York, N. Y.

Hiller, Alma, Ph.D., Biological Chemistry. Associate in Chemistry, Hospital of the Rockefeller Institute for Medical Research, New York, N. Y.

Hyman, Albert Salisbury, M.D., Med Sc.D., Zgn.Med. (Vienna), Cardiovascular Disease. Cardiologist, New York City Hospital, Beth David Hospital, Jewish Memorial Hospital; Consulting Cardiologist, Richmond Memorial Hospital, Yonkers Hospital; Director, Witkin Foundation for the Study and Prevention of Heart Disease, New York, N. Y.; Captain, Medical Corps, U. S. Naval Reserve.

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Labarre, Jules, L.Sc., D.Sc. (Paris, France), Biochemistry—Proteins and Amino Acids. Professor of Biochemistry and Pharmacodynamics, University of Montreal; Technical Director, Desbergers-Bismol Laboratories, Montreal, Canada.

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Newell, Norman D., Ph.D., Paleontology (Invert.) and Historical Geology. Professor of Geology, Columbia University; Curator, Geology and Paleontology, American Museum of Natural History, New York, N. Y.

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Paul, Andrew B., M.D., Medicine, Allergy. Chief, Medical Clinic, Reconstruction Unit, Postgraduate Medical School and Hospital, Columbia University, New York, N. Y.

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Rhodes, Robert Clinton, Ph.D., Protozoology, Parasitology, Human Genetics. Eugenics. Professor of Biology and Chairman of Department, Emory University, Ga.

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TRANSACTIONS
of
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SER. II, VOL. 8 MARCH, 1946 No. 5

SECTION OF GEOLOGY AND MINERALOGY

FEBRUARY 4, 1946

DOCTOR FRANCO RASETTI, Professor of Physics, Laval University, Quebec: *Problems of Cambrian Stratigraphy and Paleontology in Quebec.* (This lecture was illustrated by lantern slides.)

The south shore of the St. Lawrence, from a few miles above, to more than 250 miles below, Quebec City, is occupied by a complex of red and green shales, with interbedded sandstone, quartzite, and limestone conglomerate. These rocks form the main portion of Logan's classic "Quebec group," and are now usually known as the "Sillery" and Lévis formations. Logan considered the latter the older of the two, but later Ells showed that the "Sillery" underlies the Lévis. The Lévis includes the celebrated graptolite-bearing black shales, and is geographically limited to a narrow area in the vicinity of the type locality. The "Sillery" is almost totally barren of fossils in place, although Cambrian faunas of several ages occur in the conglomerate boulders.

On the basis of a few finds of graptolites and of the supposed conformable relation between Lévis and "Sillery," it has been generally assumed that the "Sillery" is Lower Ordovician or, partly, Upper Cambrian. This assumption was questioned, for the first time, by Ulrich and Cooper, who restudied the brachiopod *"Obolella" pretiosa,* described by Billings from outcrops of the "Sillery" at the type locality

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and on the Chaudière River. They concluded that the species belongs to the genus *Botsfordia* and is Lower Cambrian.

The writer, believing that no advance toward the solution of the problem could be made with the fossil evidence on hand, searched the exposures for additional fossils. This search was rewarded with the discovery of a small Lower Cambrian trilobite fauna in place, a few miles east of Lévis (Rasetti, 1945a). Fortunately, the fossiliferous horizon occurs in an excellent section of more than 1600 feet of strata in clearly recognizable order. Later, the same section yielded the graptolite *Callograptus* about 250 feet above the Lower Cambrian fossil zone (named the *Austinvillia* zone from one of the characteristic trilobites). Finally, still 1300 feet higher in the section, were collected two species of *Orthis* and a new trilobite. These finds prove that the strata in this section—all apparently in conformable succession—range in age from Early Cambrian to Early Ordovician. The shales throughout the section present little difference in lithology; hence, the presence of rocks of widely different ages had never been suspected. A few feet above the *Austinvillia* zone lies a conspicuous limestone conglomerate bed, whose boulders yield many Lower Cambrian fossils. It is tentatively assumed that this conglomerate marks the base of the Canadian. Under this assumption, the entire Middle and Upper Cambrian would not be represented in the area, although no erosional disconformity can be observed at the base of the conglomerate.

The typical "Sillery" of the Chaudière (1500 feet of red shale and sandstone are excellently exposed there, with *Botsfordia pretiosa* at the top) does not seem to be represented in the section just described; hence, it must be either entirely older or entirely younger than the beds east of Lévis. The latter assumption cannot be accepted, since the described beds east of Lévis are succeeded by the Lévis formation, with its characteristic fossils. We conclude that the beds on the Chaudière are older, hence, entirely Lower Cambrian. The writer proposes a new name, Charny formation, for these Lower Cambrian beds, since the name "Sillery" has been applied both to Lower Cambrian and to Lower Ordovician strata, and its further use would cause confusion. For the Lower Ordovician strata underlying the Lévis, the old term Lauzon, introduced by Richardson and later abandoned in favor of "Sillery," may be revived.*

* A detailed account of the stratigraphy of the Lévis-Chaudière area will be published in the Bulletin of The Geological Society of America.

A puzzling problem in the area here discussed is the origin of the numerous beds of limestone conglomerate with exotic boulders. These conglomerates occur at several places on the south shore of the St. Lawrence, and seem to be all of Early Ordovician age. Those in the Lévis shale at Lévis are celebrated for their rich trilobite faunas (Rasetti, 1944, 1945b). The others, farther east, seem to belong to the Lauzon; they are usually associated with sandstone and quartzite, often in exceedingly thick beds. The limestone boulders in these conglomerates yield a variety of Lower, Middle, and Upper Cambrian faunas; sometimes all these faunas occur in the same bed (Rasetti, 1945c). These limestones of different ages are all similar, lithologically, and also have in common the fact that no strata of corresponding lithology and age are now known anywhere in Quebec. An attempt toward the explanation of the origin of these conglomerates has been made by Bailey, Collet, and Field (1928). The strata from which the boulders came may now be buried under the overthrust mass of the Charny, Lauzon, and Lévis. In the foreland (northwest of the St. Lawrence—Champlain fault), no Cambrian strata were sedimented, the Trenton overlying the pre-Cambrian gneiss. However, the slope of the sea bottom may have been very steep, and such Cambrian strata may have existed in the area, now buried under the thrust sheet, a few tens of miles south of the margin of the pre-Cambrian shield.

Whatever the origin of the limestone boulders, the study of their faunas is worth while, because they yield a great variety of unusually well preserved Cambrian trilobites of different ages. The description of the Upper Cambrian faunas has now been completed by the writer, who is undertaking the study of the Lower and Middle Cambrian forms. A few points that are relevant for the phylogeny and classification of trilobites will be briefly discussed.

Beecher divided the trilobites into three orders, Hypoparia, Opisthoparia, and Proparia, according to the character of the cephalic sutures. In the Hypoparia, the free cheeks were supposed to be represented by a ventral plate, and the marginal suture that separates it from the cranidium was homologized with the facial suture of the other two orders. Since the eyes of proparian and opisthoparian trilobites, whenever present, are invariably situated on the course of the facial sutures, Beecher could not consider the eyes of such forms as *Tretaspis* and *Harpes* (which are situated in the middle of the cheeks and not on

the marginal suture) as homologous with the eyes of other trilobites. Several paleontologists, among them, Swinnerton, Richter, and Stubblefield, sharply criticized Beecher's order, Hypoparia, by bringing arguments to show that the eyes of *Tretaspis* and *Harpes* are homologous with the eyes of other trilobites, being situated on the fused facial sutures; whereas, the marginal suture was developed secondarily and does not correspond to the facial suture of the Opisthoparia and Proparia.

The writer believes he has discovered the most convincing case for the homology of the eyes in trilobites, with and without facial sutures. A late Upper Cambrian genus from the boulders in the Lévis shale, *Loganopeltoides*, has eyes and sutures of a peculiar shape, suggesting that the dorsal sutures are on the way to becoming reduced. An early Canadian genus from the same locality, *Loganopeltis*, is almost identical with the preceding, except that the dorsal sutures have become entirely fused; the eyes have preserved the same position. According to Beecher, *Loganopeltis* should be styled a hypoparian form; but it is almost impossible to believe that these two trilobites are not close relatives, and that the eyes in both forms are not homologous (Rasetti, 1945d).

Another group of trilobites that has attracted the writer's attention is represented by the Eodiscids. These small trilobites, chiefly characterized by the cephalon and pygidium of subequal size and by possessing only two or three thoracic segments, appeared in Early Cambrian time and became extinguished before the end of the Medial Cambrian. Here again, we have a case that shows the exaggerated importance attributed to the facial sutures by Beecher and his followers. The Eodiscids were supposed to belong to the Hypoparia, since the forms known in Beecher's time had no dorsal sutures. Walcott, however, discovered, in the Middle Cambrian of the Cordilleran province, the genus *Pagetia*, which is, in most respects, almost identical with *Eodiscus*, excepting the presence of small free cheeks of the proparian type. It is difficult to deny the close relationship between the two genera.

The Lower and, possibly, Middle Cambrian boulders in the conglomerates of the Lauzon formation yield a remarkable variety of Eodiscids, with eyes and free cheeks. Some of these have been assigned by the writer to the genus *Pagetia*, some to a new genus, *Paget-*

ides (Rasetti, 1945a). Furthermore, a restudy of certain forms from other localities showed that *Microdiscus connexus* Walcott, from the Lower Cambrian Schodack formation of New York, originally described as blind, has eyes and free cheeks; and that a species of *Pagetides* occurs in the Parker shale of northwestern Vermont. This definitely shows that trilobites with facial sutures of the proparian type, supposed by Beecher to be the most advanced, already existed in the Early Cambrian; as a matter of fact, we do not know which is the more primitive condition in the Eodiscids, the presence or the absence of eyes and dorsal sutures. It must be understood that there is not the slightest indication that these proparian Eodiscids were the ancestors of the post-Cambrian Proparia. The writer believes that the Eodiscids should constitute a separate order, since we do not know anything even remotely resembling a transitional form, between the Eodiscids and the multisegmented trilobites. The Eodiscids have long been associated with the Agnostids, but the writer agrees with most modern authors in considering the two groups as entirely unrelated.

LITERATURE CITED

Bailey, E. B., L. W. Collet, & R. M. Field
1928. J. Geol. **36**: 577–614.

Rasetti, F.
1944. J. Paleont. **18**: 229–258.
1945a. Am. J. Sci. **243**: 305–319.
1945b. J. Paleont. **19**: 462–478.
1945c. Natur. Canadien. **72**: 53–67.
1945d. Am. J. Sci. **243**: 44–50.

SECTION OF BIOLOGY

February 11, 1946

Doctor Laurence H. Snyder, Chairman, Department of Zoology and Entomology; and Professor of Medical Genetics, College of Medicine, The Ohio State University, Columbus, Ohio: *Recent Advances in Human Heredity.* (This lecture was illustrated by lantern slides.)

Advances in the basic principles of genetics have been reflected in parallel advances in the application of these principles to man. The study of human heredity has progressed slowly, because it is essentially a study of population genetics, and, as such, requires different techniques than those used in the usual experimental laboratory genetics. These techniques, involving the analysis of gene frequencies, equilibria, contingency tests, twin comparisons, and the like, have been gradually developed and perfected. As a result, there is now available a considerable amount of exact information on the role played by genetic variation in the development of diverse human characteristics. One outcome of the availability of such information is the increasing realization of the importance of a knowledge of human heredity, in modern life. The realization has given rise to the inclusion of courses in medical genetics in the curricula of various medical schools, the setting up of required courses in heredity in various curricula of law, sociology, social administration, psychology, and anthropology, and the presentation of lectures on human heredity under a wide variety of auspices.

The extent and precision of our present knowledge of human heredity warrant the formulation of practical applications to human welfare. These applications include, first, genetic prognosis, that is, the prediction of the appearance or reappearance of hereditary traits within families; second, diagnosis, on the basis of genetic data, of conditions difficult to diagnose readily on other grounds; third, the instituting of preventive measures against certain diseases and anomalies in the relatives of affected individuals; and fourth, medico-legal applications in disputed paternity and identification of individuals, based on the inheritance of specific test characters, such as the blood agglutinogens.

Examples of each of these practical applications have been repeatedly presented and discussed in the literature (*cf.* Macklin, 1940; Snyder, 1941, 1943, 1946b; Muller, Little, and Snyder, 1946; Wiener, 1943).

The basis on which practical applications are built is a body of data, painstakingly gathered and carefully evaluated, about the mutant gene, in general, and concerning the mutant gene in man, in particular. I have classified the basic relations of the mutant gene in man under four main headings: spatial relations, physiological relations, ontogenetic relations, and phylogenetic relations (Snyder, 1942). Knowledge of some of these, in regard to any gene, is essential, and knowledge of all of them is desirable, before formulating a practical application, in regard to the trait conditioned by the gene.

In man, a mutant gene may be autosomal, it may be sex-linked, it may be holandric, or it may be incompletely sex-linked. Its behavior in transmission will depend upon this absolute location. Many examples of each of these types of spatial relationships are now known in human genetics.

Two mutant genes may be independent or linked. Tentative linkage maps for man are now available, both for the sex chromosomes and for the autosomes (Snyder 1946a), and their further extension may be expected to add precision to genetic prognosis and to lead to the early recognition of preclinical symptoms and, thus, to the institution of preventive measures.

Physiological relations of the mutant gene include penetrance, expressivity, and viability. These are all variable, from gene to gene, and require special methods of analysis. A knowledge of these relations is important to genetic prognosis.

Ontogenetic relations include dominance, recessivity, epistasis, hypostasis, and cumulativeness. These are the usual "Mendelian" relationships. They have some special applications in genetic prognosis.

Phylogenetic relations include the frequencies of a gene and of its alleles in the population, and the frequencies of the genotypes resulting from combinations of the gene and its alleles. It is at this point that the study of human genetics differs from the usual laboratory analysis, where the employment of experimental matings insures that the genotype of each individual is known or can be determined. In man, the genotypes of individuals, in the families under observation, are capable, in most cases, only of incomplete specification, and the study of human genetics becomes, in the final analysis, a study of population genetics. Basic statistical researches by many workers, both in this

country and abroad, are constantly improving our methods of analysis of the problems of gene frequencies, equilibria, mutation pressure, selection pressure, and allied phenomena (*cf.* Dobzhansky, 1941; Snyder, 1942, 1946a). Through such basic researches and the careful collection and analysis of human data, the study of human heredity is making rapid strides.

LITERATURE CITED

Dobzhansky, T.

1941. Genetics and the Origin of Species. Second edition. Columbia University Press. New York.

Macklin, M. T.

1940. Medical Genetics and Eugenics. **I** (6). Women's Medical College of Pennsylvania. Philadelphia.

Muller, H. J., C. C. Little, & L. H. Snyder

1946. Genetics, Medicine and Man. Cornell University Press. Ithaca.

Snyder, L. H.

1941. Medical Genetics. Duke University Press. Durham.

1942. The mutant gene in man. American Naturalist. **76**: 129.

1943. Medical Genetics and Eugenics. **II**. Women's Medical College of Pennsylvania. Philadelphia.

1946a. The Principles of Heredity. Third edition. D. C. Heath & Co. Boston.

1946b. The Rh factor in feeblemindedness and other diseases. Minnesota Medicine. **29**: 121.

Wiener, A. S.

1943. Blood Groups and Transfusion. Third edition. Charles C. Thomas. Springfield, Ill.

SECTION OF PSYCHOLOGY

FEBRUARY 18, 1944

DOCTOR KURT LEWIN, Department of Economics and Social Science, Massachusetts Institute of Technology, Cambridge, Massachusetts: *Research in Group Dynamics.*

(No abstract received.)

SECTION OF ANTHROPOLOGY

February 25, 1946

Doctor Sherwood L. Washburn, Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.: *Experimental Anthropology.*

Anthropology is changing rapidly. The last ten years have seen the rise of culture and personality studies. The increased application of anthropological knowledge, during the war, has given a tremendous impetus to applied anthropology.

The dominant emphasis of traditional anthropology, particularly in the United States, was history and explanation of the present in terms of the past. Today, the effort is being made to understand social and physical processes, to analyse in terms of general principles (such as the laws of psychology or genetics).

The change, from a descriptive-historical to an analytic approach, has removed the boundaries of traditional anthropology. Description can be limited to primitive peoples and primates, but psychological principles apply to all mankind, just as biological laws apply to all animals.

Many of the theories which attempt to explain evolutionary change can be tested in the laboratory. Experiments simplify, clarify, and offer new insights into complicated natural processes.

The way in which experiment enters into the theoretical scheme of physical anthropology may be described as follows: The primates are described. Theories attempt to explain what is observed. Then, experiments are performed to test the validity of the theories.

The complementary role of historical and experimental studies may be illustrated by consideration of the human orbit. If one wishes to know the stages through which the orbit has evolved, one must study the primates, particularly the fossils. If one wishes to know what forces shape the orbit, one must experiment. Each type of analysis supplements and enriches the other.

Frequently, when experiments are performed, unsuspected factors are discovered, and these make possible new approaches to history. The experiments which led to the theory of genetics, revolutionized the

theoretical basis of evolution. (The theoretical scheme was illustrated by slides of rat skulls, showing the effects of removal of various muscles and bones.)

There is a strong tendency for sciences to be divided, so that descriptive studies are separated from the complementary and experimental sciences. Anthropology should make every effort to include both forms of knowledge. No thorough understanding of man or his culture is possible without history and experiment.

SECTION OF BIOLOGY

FEBRUARY 8 AND 9, 1946

Conference on *"The Physico-Chemical Mechanism of Nerve Activity."*

The Section of Biology held a Conference on "The Physico-Chemical Mechanism of Nerve Activity," arranged by Doctor David Nachmansohn, College of Physicians and Surgeons, Columbia University, New York, N. Y.

The program consisted of the following papers:

FRIDAY, FEBRUARY 8

Morning Session. Chairman, Tracy J. Putnam, College of Physicians and Surgeons, Columbia University, New York, N. Y.

Opening Address, by Tracy J. Putnam.

"Membrane Theory," by Rudolf Hoeber, University of Pennsylvania, Philadelphia, Pennsylvania.

"Chemical Mechanism of Nerve Activity," by David Nachmansohn.

Afternoon Session. Chairman, John F. Fulton, Yale University, School of Medicine, New Haven, Connecticut.

"An Electrical Hypothesis of Synaptic and Neuromuscular Transmission," by John C. Eccles, University of Otago, Dunedin, New Zealand.

Evening Session. Chairman, Frederic Bremer, University of Brussels, Belgium.

"Chemical Activation of Nervous Function," by Detlev Bronk and Frank Brink, Jr., University of Pennsylvania, Philadelphia, Pennsylvania.

"Electric Characteristics of Electric Tissue," by Richard T. Cox, C. W. Coates, and M. V. Brown, Johns Hopkins University, Baltimore, Maryland; New York Zoological Society; and College of the City of New York, New York, N. Y.

SATURDAY, FEBRUARY 9

Morning Session. Chairman, McKeen Cattell, Cornell University Medical College, New York, N. Y.

"Enzymes as Means of Studying Cellular Function," by David E. Green, College of Physicians and Surgeons, Columbia University, New York, N. Y.

"Cholineesterase," by Oscar Bodansky, Medical Division, Chemical Warfare Service, Edgewood Arsenal, Maryland.

"Effects of Drugs on Nerve Activity," by Alfred Gilman, Medical Division, Chemical Warfare Service, Edgewood Arsenal, Maryland.

Afternoon Session. Chairman, Francis O. Schmitt, Massachusetts Institute of Technology, Cambridge, Massachusetts.

"Regeneration of Nerve Fibers," by Joseph Hinsey, Cornell University Medical College, New York, N. Y.

"Metabolism and Function," by Ralph W. Gerard, University of Chicago, Chicago, Illinois.

SECTION OF BIOLOGY, AND SECTION OF PHYSICS AND CHEMISTRY

February 15 and 16, 1946

Conference on *"Muscular Contraction."*

The Sections of Biology, and Physics and Chemistry held a Conference on "Muscular Contraction." Doctor Alexander Sandow, New York University, Washington Square College of Arts and Pure Science, New York, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Friday, February 15

Morning Session: Dynamics. Chairman, Alexander Sandow.

General Introduction, by Alexander Sandow.

"Dynamics of Single Muscle Fibers," by Robert W. Ramsey, Medical College of Virginia, Richmond, Virginia.

"The Time Course of Tension Development in the Muscle Response," by A. S. Gilson, Jr., G. M. Schoepfle, and S. M. Walker, Washington University, School of Medicine, St. Louis, Missouri.

Afternoon Session: Ultrastructure. Chairman, John T. Edsall, Harvard Medical School, Boston, Massachusetts.

"Birefringence and Ultrastructure of Muscle," by Ernst Fischer, Medical College of Virginia, Richmond, Virginia.

"Electron Microscope and X-Ray Diffraction Studies of Muscle Structure," by Francis O. Schmitt, R. S. Bear, C. E. Hall and M. A. Jakus, Massachusetts Institute of Technology, Cambridge, Massachusetts.

"Muscular Contraction and Rubber-like Elasticity," by E. Guth, University of Notre Dame, Notre Dame, Indiana.

Saturday, February 16

Morning Session: Chemistry. Chairman, Carl F. Cori, Washington University, School of Medicine, St. Louis, Missouri.

"The Main Chemical Phases of the Recovery of Muscles," by Otto Meyerhof, University of Pennsylvania, Philadelphia, Pennsylvania.

"Chemical Processes of Oxidative Recovery," by Severo Ochoa, New York University, College of Medicine, New York, N. Y.

Afternoon Session: Mechano-Chemical Coupling. Chairman, Wallace O. Fenn, University of Rochester, School of Medicine and Dentistry, Rochester, New York.

"Intracellular Cations and Muscle Actions," by H. B. Steinbach, Washington University, St. Louis, Missouri.

"The Potential Role of the Myosin-ATPase-Calcium Complex in the Activation and Energy Output of Muscle," by Dugald Brown, New York University, College of Dentistry, New York, N. Y.

"Latency Relaxation and a Theory of Muscular Mechano-chemical Coupling," by Alexander Sandow, New York University, Washington Square College, New York, N. Y.

NEW MEMBERS

Elected February 28, 1946

SUSTAINING MEMBERSHIP

Browne, Dudley, Chemistry, Pharmacology, Biology. Asst. Vice President and Research Coordinator, American Home Products Corp., New York, N. Y.

Holm, August, Ph.D., Sc.D., Bacteriology, Immunology. Head, Bacteriological Development Laboratories, E. R. Squibb & Sons, New Brunswick, N. J.

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Atlas, Meyer, Ph.D., Physiology. Asst. Professor, Biology, Yeshiva College, New York, N. Y.

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Blubaugh, Louis V., Ph.D., Biology. Asst. Director, Products Development, E. R. Squibb & Sons, New Brunswick, N. J.

Brown, George Bosworth, Ph.D., Biochemistry. Associate, Sloan-Kettring Institute for Cancer Research; Research Associate, Cornell University Medical College, New York, N. Y.

Brown, Jean Cameron, M.A. Asst. Geologist, American Metal Co., Limited, New York, N. Y.

Brunings, Karl J., Ph.D., Organic Chemistry. Asst. Professor of Chemistry, New York University, New York, N. Y.

Carroll, William R., Ph.D., Biochemistry, Physiology. Research Associate, Department of Biochemistry, Cornell University Medical College, New York, N. Y.

Chambers, Robert, Ll.D., Ph.D., Experimental Biology. Research Professor, Biology, New York University, New York, N. Y.

Chambers, William H., Ph.D., Metabolism, Calorimetry, Nutrition. Associate Professor, Physiology, Cornell University Medical College, New York, N. Y.

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Clark, Leland C., Jr., Ph.D., Biochemistry, Physiology. Research Associate and Chairman, Biochemistry Department, Fels Research Institute, Antioch College, Yellow Springs, Ohio.

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TRANSACTIONS

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SECTION OF GEOLOGY AND MINERALOGY

MARCH 4, 1946

DOCTOR H. T. U. SMITH, Geologist, U. S. Geological Survey, Washington, D. C.: *Sand Dunes*.[1]

In the United States, sand dune areas cover more than 40,000 square miles, or approximately 1.4 per cent of the total land area of the country. The largest areas are in the Great Plains, mainly in Nebraska, Kansas, and eastern Colorado. Smaller areas occur in intermontane basins and plateaus, in the Pacific coastal area, along the southern and eastern shores of Lake Michigan, in adjoining lake states, and along the Atlantic Coast.

Primary dune forms may be grouped in two main classes: (1) those in bare, free sand, and (2) those developed in the presence of vegetation. The former occur mainly in desert regions, and comprise barchans, transverse dune ridges, certain varieties of longitudinal dunes (dunes parallel to wind direction), wind-shadow dunes controlled by topographic obstacles, and various irregular forms. Of these, the barchans and transverse dunes are migratory, the others essentially non-migratory. In the desert areas of western United States, transverse dune ridges are, perhaps, the commonest type, and barchans, as in other desert areas, are comparatively rare. In the Sahara, however, longitudinal dunes are more prevalent, and these show considerable diversity both in size and in details of form. There also are found cer-

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tain special dune forms, the "fulji," or deep depression resembling the imprint of a giant horse's hoof, and the "rhourd," or rudely pyramidal stationary dune with radiating, sharp-crested spurs.

Dunes of the second class, those developed in the presence of vegetation (phytogenic), occur in coastal areas and in semi-arid plains regions, and comprise U-, V-, and Y-shaped forms, various modifications and combinations of these, certain varieties of longitudinal dunes, "foredunes" bordering sandy beaches, and minor irregular forms. Dunes of this class, although subject to growth in a down-wind direction, are essentially non-migratory. Their development is best described in terms of an ideal dune cycle, comprising eolian and eluvial phases. In the eolian phase, wind action is dominant and sand is blown from bare source area to a bordering zone of accumulation, where vegetation checks sand drift. Where the vegetation is of such a type as to resist choking by sand, its upward growth progressively traps more sand, and a definite mound or ridge develops. As the sand accumulates, variations in the resistance offered by the vegetation may lead to localized "break-through" of the sand, permitting differential erosion and transportation of sand and consequent formation of a bulge or salient in the dune form. Under favorable conditions, the growth and extension of this salient may entirely overshadow the antecedent form.

The eolian phase may be ended, at any stage, through stabilization of the dune and source area by vegetation. Thereafter, gradual degradation by soil building, creep, rainwash, and related processes constitutes the eluvial phase of development. Contours are rounded, hollows are filled, slopes are lowered, relief is reduced, the soil zone thickens, and, at some stage, thorough-going surface drainage may be established. Dune forms become less and less distinct and finally unrecognizable, as the topography passes through stages of youth, maturity, and old age of the eluvial phase.

The eluvial phase may be interrupted, at any stage, by breaking of the vegetal cover and renewal of wind attack, thus initiating the eolian phase of a new cycle. Primary dune forms are dissected by secondary blowouts, and a wide range of forms may develop.

Continued alternations of eolian and eluvial development lead to increasing topographic complexity, and in many areas, the details of geomorphic history become undecipherable. In the semi-arid Great Plains

of this country, examples of all stages of the above scheme of development are found; the major period or periods of dune building, however, belong to the past, and wind action, at present, is on a very small scale. Dunes are predominantly "fossil" forms, also, in the more humid areas of western Europe, but complexity due to multi-cycle history is less common.

Correction

In Transactions for February (Vol 8, No. 4) page 168 at the end of the footnote, instead of "the most recent prize was awarded in 1938 to Hans Bethe" read "the most recent prizes were awarded in 1938 to Hans Bethe and in 1940 to R. E Marshak and Hans Bethe"

SECTION OF BIOLOGY

March 11, 1946

Doctor F. Duran-Reynals, Yale University School of Medicine, New Haven, Conn.: *Virus Variation in Relation to the Cancer Problem.* (This lecture was illustrated by lantern slides.)

Cancer, like so many other diseases, seems to occur in some species preferably to others. We can, thus, speak of human cancer, murine cancer, and avian cancer, as the three main groups. The latter offers many analogies with mammalian cancer, with the added feature that viruses, as causative agents, are easily demonstrable in many tumors.

It has been shown that sarcoma viruses of chickens, with infecting embryos or chicks, do not induce cancer, but induce a destructive disease of a hemorrhagic sort, comparable to the disease caused by inflammatory, necrotizing viruses. Fundamentally, the same is true for the viruses of chicken lymphomatosis, rabbit fibroma, and sheep pox. So, we have a group of "neoplastic" viruses, which behave as neoplastic only when infecting adult hosts. The reason for this duality of effect is to be found in the extreme susceptibility of the embryo and the young, in contrast to the resistance of the adult and the old. Treatment of young chicks infected with a sarcoma virus with serum from old chickens makes the chick react to the virus much as adults do.

Further, it has been shown that chicken tumor viruses are endowed with the property of varying or mutating in association with the infection of foreign species, but this property is only manifest when the virus comes from a tumor, grown in an adult or old host, which provides an environment somewhat adverse to the virus, without, as yet, reaching the point where the environment is so unfavorable as to make large tumors regress. The phenomenon is analogous to the development of variants in ageing bacterial cultures. Other manifestations of the influence of age on cancer are the difficult transplantability of either spontaneous or transplantable tumors, and the lack of filtrability of the latter tumors from old hosts.

In summary, old age, on the one hand, offers a ground favorable to mutation or variation of viruses into cancer viruses, and on the other hand, is a ground fundamentally adverse to the development of

infection. Indeed, if the resistance of the old were just a degree higher, then complete resistance against cancer viruses would be achieved. Neoplastic manifestation of some viruses and variation of these viruses are the first steps leading to tumor suppression.

SECTION OF PSYCHOLOGY

MARCH 18, 1946

DOCTOR H. L. HOLLINGWORTH, Professor of Psychology, Columbia University, New York, N. Y.: *Experimental Psychology and Ethics.*

In the report to be made here, we are interested, not in the bearing of ethics on the activities of experimental psychology, but in the application of certain experimental methods to ethical topics. Ethics has, of course, an old-fashioned sound, and some may feel that experimental psychology should occupy itself with livelier issues. There are, however, cogent reasons for reviving an interest in ethics.

Those called on to offer guidance to the young now find that they often lack adherence to authoritarian codes, such as their parents knew. They also lack the reflective experience, which their parents may have had, in the historical and critical study of ethical systems. How can they, then, be counselled, lacking both faith and dialectic?

There are also questions concerning the implications of contemporary systems of psychology for moral problems and for the explanation of imperative behavior. For there are imperative acts and there is a sense of obligation. How does your favorite system of psychology handle them? Or does it sidestep such issues as value?

Finally, there are notable individual differences in the appreciation of ethical demands, in the discrimination of varieties of obligation, and in the readiness to assume moral responsibility. Should we not have better instruments for early diagnosis and measurement of such peculiarities and for their correlation with other personality traits? It is with such questions in mind that we have undertaken to approach certain ethical topics in a somewhat experimental fashion.

A survey has been made of the psychological basis of the sense of obligation and of individual differences in insight into the types of imperative conduct. In a preliminary inquiry, adults were asked to classify imperative propositions (statements containing the word *ought*) into as many categories as there seemed to be. They were then asked to describe or characterize these groups or categories. Ten major categories were revealed: the imperatives or oughts of Inference,

Gestalt, Esthetics, Social Welfare, Utility, Duty, Safety, Convention, Justice, and Legislation.

These varieties of imperative appear to include the whole of human behavior, so that obligation is, in a sense, characteristic of all our acts and conclusions, and value is not a dissociated realm of experience. These chief categories, differing as they may in subject matter and genesis, have in common the assertion of a particular item as the effective technique for the mitigation of misery, for the relief of a distress that is either expressed or implied.

The systems of psychology known as structuralism, behaviorism, gestalt theory, and purposivism do not provide adequate basis for the understanding of these fundamental imperatives, nor for the basis of the sense of obligation. Structuralism is interested only in the momentary, sensory description of an experience of obligation, with no concern for genesis or for values. Behaviorism, in its naïve historical form, pays attention only to sequence of stimulus and movement. It might distinguish positive from negative reactions and between native and learned connections, as well as between slow and rapid sequences. But the obligation, the imperative, could, for this point of view, be nothing more than the coerciveness of a reflex; the feeling of obligation, along with all other feelings, would have to be repudiated. For more sophisticated and contemporary behaviorists, conduct has, for its outcome, what is called need-reduction. But needs are described as the lack of commodities, and the mere non-existence of something would appear wholly unable to produce anything in the way of action. It is not the absence of a commodity, but the positive presence of a misery, that motivates conduct.

Gestalt theory is so pre-occupied with the stresses and strains of field dynamics that it overlooks the actual individual distresses that motivate our acts and give force to the facts of what it calls "requiredness." The alleged instincts of the hormic psychology do not accord, in any clear fashion, with the list of fundamental imperatives. Even if they did, they would be wholly *ex-post facto* ways of describing our behavior, not ways of producing it. The purposivist's "sentiments" are more flexible, in this respect, since any kind of a sentiment can be formed about any kind of a thing. But the sentiments, also, are merely convenient summaries of the ways we act, not explanations of why we do so.

Our analysis leads to three fundamental principles that have been

found useful in summarizing psychological facts, in various other fields, and are found to be equally useful in this connection. These are the principles that: (1) All motives are distresses; (2) Learning is cue reduction; and (3) Control is a function of scope.

Applied to the analysis of the fundamental varieties of obligation, these principles yield a theory of value and a system of ethical evaluation. They give a helpful account of the nature of the good, the criteria of the moral, the meaning of happiness; and show the importance, also, of socially recognized hierarchies of distress and of techniques of alleviation. Such an account is offered as a substitute for the cruder historic systems of hedonism, asceticism, utilitarianism, intuitionism, authoritarianism, logical idealism, and the like.

Important in this account, among other features, is the rejection of the classical, bi-polar theory of motivation and affection. Distresses are conceived as primary, and pleasure is the relief from initial and instigating distress. Happiness, moreover, is not mere absence of distress; it is the possession of a repertoire of effective techniques for eliminating the distresses incident to being alive. The supreme happiness is the level of playful mastery on which the individual seeks out or devises difficulties for the sheer pleasure of mastering them. Such a viewpoint emphasizes the role of education as against that of social reform, and encourages the individual to be the instrument of his own salvation.

The moral quality of an act is determined by the completeness, the promptness, and the permanence with which it relieves the distress which is its motive, with a minimum of correlated misery in any creature, and by the recognized level of that irritant and of that technique in the relevant, individual and social hierarchies of distresses and of techniques.

The distresses underlying the imperatives of logical inference are, of course, such miseries as doubt, uncertainty, and curiosity. Basic to the gestalt imperatives, are the discontents of incompleteness and inappropriateness. Esthetic imperatives relate to the relief of such dissatisfactions as those of ugliness, discord, and asymmetry. Imperatives of Welfare and of Safety are motivated by such calamities as pain, anxiety, hunger, failure, illness, and similar varieties of suffering, including the pangs of sympathy. The oughts of Utility show how to avoid waste and defeat; those of Convention, how to avoid social chagrin, and those of Legislation, how to escape the inconveniences of legal en-

tanglement. Under the category of Duty, come all the 'oughts' based on codes, ideals, hero worship, religious precepts. The imperatives of Justice have not been given adequate psychological attention. Apparently, it is the feeling of injustice, rather than a sense of justice, that really operates. The experience of injustice may be based on the kinesthetic appreciation of imbalance in the tensions of our bi-laterally symmetrical musculature.

Under any circumstances, what we commonly call *conscience* may underlie the imperatives of any of these categories. To all of them, also, we find such words as good and bad, right and wrong, equally applicable. It is for such reasons that we advocate the extension of such terms as moral and ethical to all of these classifications and obligations. We find no essential difference between the 'oughts' of expectation, the 'oughts' of responsibility and the 'oughts' of necessity, although individuals differ importantly in their capacity to appreciate such differences as do exist.

Tentative experimental methods or tests have been devised for the measurement of individual differences in moral insight, as revealed by the appreciation of these ten fundamental categories. Typical results are now available from such methods, in the case of several hundred persons varying in age, education, and intelligence. Several alternative procedures have been compared, and a form chosen which seems to differentiate on various developmental levels and to require not more than half an hour for its administration. Quantitative scores ranging from zero to one hundred points make comparative study possible.

The range of moral insight, as thus assessed on the same educational or intelligence level, is surprising. So, also, is the relatively advanced level required before a clear understanding and recognition of these categories of obligation is achieved. The existence of such marked individual differences and the qualitative moral confusions exhibited by individuals suggest the desirability of improved methods of moral instruction. The high school years are indicated as the appropriate time for such instruction and measurement. A feasible course of study may be based on the analysis and comparison of the ten fundamental categories. Thus, the obligations or 'oughts' of esthetics may be surveyed; likewise, the oughts of utility, of convention, and so on. These departments may, then, be contrasted, and practice may be given in their recognition.

Insight of the kind here measured appears to follow a developmental curve which it would be valuable to know more about in detail. Attempts are now being made, not only to perfect the procedure and materials, but also to secure results from groups varying widely in developmental status, in the endeavor to construct such a curve.

In these preliminary experiments, several rather different procedures have been tried out and compared. Two sheets are here reproduced, constituting Plan 4, which appears to be the best of those yet tried out. The first sheet is a Set of Instructions, with illustrative examples for each of the fundamental categories emerging from the results secured by Plan 1. The second sheet gives 50 imperatives (statements containing the word *'ought'*), which are to be classified according to the Instructions. Standards of correctness derived from the concensus of opinion of a group of sophisticated adults enable each classification to be scored right or wrong. Two points credit are given for each correct classification, according to this standard key.

Other plans tried out experimentally have the following characteristics: Plan 1: only statements provided, the subject to discover his own categories and classify the statements under them. Plan 2: names of ten standard categories given, and a brief definition of each, with no examples. Plan 3: ten sets of two propositions, each pair illustrating one of our standard categories, but the category is neither named nor defined. The plan finally used, and illustrated here, is essentially a combination of plans 2 and 3, and appears to have many advantages. Plan 1 had to be abandoned, because of the inability of even educated adult subjects to handle it.

A Sample Instruction Sheet (Plan 4)

INSTRUCTIONS

In PART I are given several varieties of OUGHT or kinds of obligation. Each is indicated by a capital letter, by a brief description, and by a sample proposition. Read this list carefully.

Now take PART II. On it are 50 propositions, all containing the word 'OUGHT.' In front of each proposition, put the capital letter showing to which variety or category of OUGHT that proposition belongs. The description and sample of PART I may be referred to for guidance. If you need to, you may put more than one letter before a proposition, but in that case, draw a line under the letter that you think fits best.

Part I. CATEGORIES

B—BEAUTY. Esthetic sense or personal taste prefers it.
"This green hat ought not to have a blue ribbon on it."

C—CUSTOM. Conventional propriety, etiquette or custom requires it.
"Those in mourning ought to wear black."

D—DUTY. Religious scruples, conscience or ideals are involved.
"Children ought to honor and obey their parents."

G—COMPLETENESS. Some occasion, situation or pattern calls for it.
"Such music ought really to be played on a pipe organ."

I—INFERENCE. Certain premises or facts lead logically to it.
"The morning is clear; we ought to have a fine day."

J—JUSTICE. Fairness and equity demand that this be.
"Teachers ought to be willing to work for small salaries."

L—LAW. Legislation, police rules, or statutes require this.
"If you carry a pistol you ought to have a police permit."

S—SAFETY. For the hygiene, success, safety, or well-being of the person.
"There ought to be a guard rail on these stairs."

U—UTILITY. Expediency, usefulness, or efficiency are the reasons.
"You ought to cover these bulbs before cold weather."

W—WELFARE. To make the world better for people in general.
"People who are closely related ought not to marry."

PART II. PROPOSITIONS

1—We won the game and we ought to celebrate.
2—That picture ought to have a wider frame.
3—Every man ought to have a chance to work for his livelihood.
4—A room like this ought to be painted a lighter color.
5—In introducing people the younger ought to be presented to the older.
6—To vote in November you ought to be over 21 years of age.
7—Everyone ought to keep his promises.
8—Capital punishment ought to be abolished.
9—This pudding ought to have more sugar in it.
10—One ought to be careful in the choice of friends.
11—You ought not to labor on the Sabbath day.
12—Those driving cars on public roads ought to have drivers' licenses.
13—You ought not to dive with your eyes shut.
14—Real estate owners ought to be responsible for taxes assessed against their property.
15—The wedding ring ought to be worn on the third finger of the left hand.
16—The school term ought to be longer than it is.
17—If Tuesday was Christmas this ought to be New Year's Day.
18—Stripes in a fat man's suit ought not to be horizontal.
19—Some cure for that disease ought to be discovered.
20—Income tax ought to take into account a man's age and obligations.
21—A man like that ought to be whipped.
22—Defective vision ought to be detected at an early age.
23—Every dog ought to be entitled to two bites.
24—The average room temperature ought to be about 68 degrees.
25—The fish ought to bite well this morning.
26—Women and men ought to receive the same pay.
27—One ought not to take revenge into his own hands.
28—A fur coat ought to cost more than a woolen one.

29—According to traffic signs cars on this road ought not to go over 25 miles an hour.
30—Every family ought to live within its income.
31—People who are going to be married in New York ought to secure a marriage license from the proper authorities.
32—That black cover ought to have a gold border.
33—You ought to wear heavier clothing in the winter.
34—The strong ought to bear the infirmities of the weak.
35—We ought to catch the bus if we take this short-cut.
36—A man ought to tell the truth regardless of consequences.
37—This knife ought to be sharpened.
38—This axe ought to have a longer handle.
39—The garden soil ought to be cultivated often but not too deep.
40—Arithmetic ought to precede algebra.
41—Garden peas ought to be soaked thoroughly before planting.
42—You ought to have your hair cut.
43—A buck saw ought to be loosened up a little when not being used.
44—People ought to mate with those whose traits are similar to theirs.
45—When walking with a lady a man ought to take the outside next to the curb.
46—You ought to have seen him make a fool of himself.
47—Where there is so much smoke there ought to be some fire.
48—With a full dress suit a man ought to wear a white tie.
49—A good classification ought to provide for all the actual cases.
50—In America a driver ought to keep to the right side of the road.

Illustrative results, from college juniors and seniors, may be cited as typical for that level of intelligence and education. Using one of the plans finally chosen as most workable, the scores from the classification under appropriate moral categories of 50 statements containing the word "ought" range from 68 to 94 points, 2 points credit being given for each correct classification. The median score for this college group is 82.

High and low groups, according to scores on intelligence tests, give different medians, the difference being 12 points, when the upper half is compared with the lower half. Reliability, as shown by the correlation of two slightly different forms of the test, is .85.

There is a correlation of .50 with scholastic aptitude tests taken in freshman year. However, students with the same scholastic aptitude scores, say near 500, which is average, range in insight scores from 60 to 90, that is, they vary as much as the entire unselected group. The indication, then, is that, while there is a positive correlation between such scores as those here considered and verbal intelligence scores, the

two are far from being identical. One of the problems has to do with the nature of the other determinants, aside from intelligence scores.

Using the same plan, representative high school students in grades 10 and 11 give a median score of only 54 points; half of the cases fall below the lowest score for the college group. A group of high school students, from a school laying special emphasis on moral instruction, gave a better median (62 points), and scores in the lower brackets were wholly missing. High school students also made a somewhat different use of the ten possible categories, as compared with college students, and this difference deserves more careful qualitative analysis.

The distributions of scores for these three groups are shown in FIGURE 1. The graphs are placed on the same base scale at the bottom of the figure, and the median scores for each group are indicated on the base lines of the respective graphs. The numbers of cases are small, but the distributions are fairly regular. Curiously enough, when the three graphs are compared it will be observed that the column showing the mode of each group represents the column in which are to be found the lowest scores of the group just above.

The distributions of the classifications over the various categories and the analysis of individual instances reveal extremely confused comprehension of the principles on which obligation is based. Boys and girls, so far as at present determined, give similar scores, and there is no clear age difference from 15 to 18. It is such results that suggest high school as the appropriate place for the introduction of a new type of course of instruction in the principles involved in moral obligation.

Several plans have been tried out in the devising of a quantitative test, the plans differing in the Instruction sheet. The one here reproduced has been found to have several distinct advantages, and further studies are to be based, in general, on this form of instruction. It may appear also desirable to revise some of the items in the test sheet, and, perhaps, to reduce the number of items employed.

In the form here given, and with the 50 items on the present test sheet, crediting 2 points for each correct item, the median score of presumably average high school students (10th and 11th grade) is near 50 points; the median score of specially selected high school students in similar grades is near 60 points; the median score by juniors and seniors in Barnard College is near 80 points. There is indication that this plan can be used lower down in the educational scale, but scores from pupils in elementary schools are not yet available.

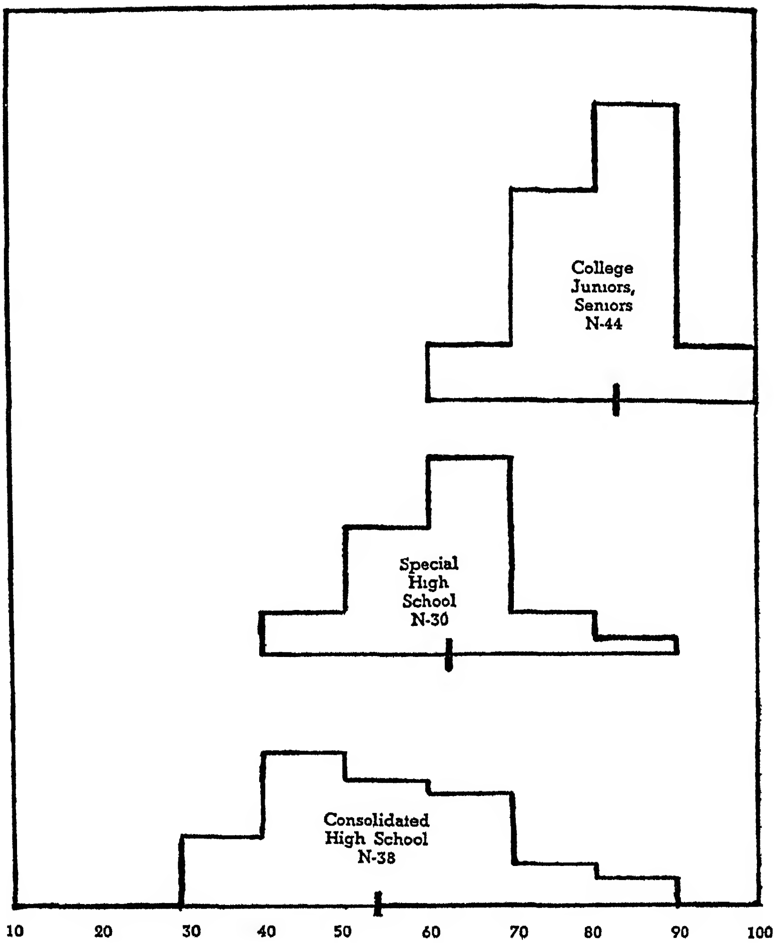

FIGURE 1. Distribution of Scores and Medians of three comparable groups.

It is believed that such tests may not only serve the purpose of initial diagnosis of degree of ethical insight, but may also be used to measure the progress made, as the result of special moral instruction, increase in age or education, and the like.

Of our preliminary forms, Plan 2 and Plan 4 are found to give the same scores on comparable groups. Combining all the groups that used either of these two Forms gives our most reliable figures for the

high school and the college groups, respectively. These figures are as follows:

	High School	*College*
Highest score	88	98
75 centile	66	86
Median Score	58	84
25 centile	46	76
Lowest Score	30	54
Number of Cases	68	70

The four quartiles, from high to low, might be designated by letter grades, such as A, B, C, and D. These letter grades might be explained as meaning, Superior, Good, Fair, and Poor. Tentative scales, based on the table just given, would then be as follows:

	High School	*College*
Top Quartile, SUPERIOR.	67–88	87–98
Second Quartile, GOOD...	58–66	84–86
Third Quartile, FAIR.....	46–57	76–83
Lowest Quartile, POOR....	30–45	54–75
Lower Scores, FAILURE..	Below 30	Below 54

In scoring, the individual's classification of the 50 items is compared with the standard key. Credit of 2 points is given for each correct item. When more than one answer is shown by the key to be correct, credit is given if either of these is indicated.

The degree of confusion experienced, even by fairly sophisticated people, in trying to make these discriminations may be briefly illustrated by a few random samples from the papers of college juniors and seniors. Here, for example, is one who thinks that the imperatives that determine the color of a room, the length of the school term, the sequence of arithmetic and algebra, and the mating of people with similar traits, all belong together under the category of Justice. Wearing the wedding ring on the conventional finger, she classifies under Beauty; loosening up an unused buck-saw belongs under Safety and Hygiene; and it is because of Social Welfare, that where there is so much smoke there ought to be some fire. On the other hand, telling the

truth, regardless of consequences, is classified as a Convention. What degree of moral understanding is possessed by such a person?

But she is not alone. Here is another who thinks that the imperatives dealing with the length of the school term, the occurrence of the New Year a week after Christmas, the presence of fire where there is so much smoke, the cost of a fur coat, and the desirability of using a short cut in order to catch the bus, all belong under the category of Esthetics, and she calls them all cases of Beauty.

Even more grotesque results may be found among the high school classifications, but there is no need to exploit them here. Such errors are not to be excused by the fact that sometimes one can see what lies behind them. Something always lies behind a boner, but usually it is just plain ignorance. Ignorance, in the cases we are here considering, means lack of moral judgment, weak ethical insight, clouded perception of the motives of conduct.

This method differs from previous studies of ethical insight in that principles of evaluation are considered, rather than items of conduct. The procedure is based on the conviction that, no matter how decent conduct may be, it does not really have moral quality without insight into the principles underlying the imperatives (obligations) that justify it. The present experimental results are tentative only, but they appear to deserve elaboration, repetition, and application to a larger number of cases and a greater variety of circumstances. Correlation of the quantitative scores with other personality measurements would also be interesting.

Regardless of the validity of these tentative quantitative results, the qualitative analysis of the meanings of ought and the comparison of the list of standard categories of obligation are believed to constitute a useful and new approach to problems of moral evaluation. Numerous circumstances and peculiarities of our present era give a new approach to ethics a very genuine value.

Another Sample Instruction Sheet (Plan 3)

INSTRUCTIONS

In PART I, are given several varieties of OUGHT or kinds of *obligation*. Each is indicated by a capital letter and two samples are given for each. The samples belong together, because they illustrate the same kind of OUGHT. First read this list through carefully.

Now take PART II. It contains 50 new propositions, each containing the word 'OUGHT.' In front of each proposition put the key letter (B or C or D etc.)

showing with which pair of samples on the Instruction Sheet the new proposition belongs. Consult the Instruction Sheet as often as you like, and take all the time you need. When in doubt, do the best you can. If you need to, you may put more than one letter in front of a proposition, but in this case *underline* the one you think fits best.

Part I. SAMPLES

B—This green hat ought not to have a blue ribbon on it.
To be pleasant this room ought to have windows on two sides.

C—Those in mourning ought to wear black.
The guest of honor ought to be seated at the host's right.

E—Children ought to honor and obey their parents.
We ought not to covet our neighbor's possessions.

G—Such music ought really to be played on a pipe organ.
Children ought to be seen and not heard.

I—The morning is clear; we ought to have a fine day.
This ought to be a better automobile than that.

J—Teachers ought to be willing to work for small salaries.
Anyone ought to be satisfied with 80 years of life.

L—Your car ought to have a license plate on it.
If you carry a revolver you ought to have a police permit.

S—Everyone ought to sleep at least eight hours a day.
There ought to be a guard rail on these stairs.

U—You ought to grip the handle more loosely.
You ought to cover these bulbs before cold weather.

W—People who are closely related ought not to marry.
Feeble-minded and insane ought to be prevented from having children.

Part II. PROPOSITIONS

1—The morning is clear; we ought to have a fine day.

2—This ought to be the road to Boston.

3—Children ought to obey their parents.

4—This ought to be about where I lost it.

5—They ought not to impose such burdens on the young.

6—We ought to render unto Caesar the things that are Caesar's.

7—Teachers ought to be willing to work for small salaries.

8—Children ought to be seen rather than heard.

9—The war ought to be over in a year.

10—That ought to be plenty of material for an overcoat.

11—Anyone ought to be satisfied with 80 years of life.

12—This ought to be a better automobile than that.

13—The pie ought to be done by this time.

14—We ought to start today if we want to be there by Friday.

15—A bed built like that ought to be very comfortable.

16—This piece of string ought to be long enough.

17—Eight tons of coal ought to heat that house for the winter.

18—Youth ought to be patient with the prejudices of the aged.

19—With good treatment this pen ought to last a life-time.

20—You ought to have been able to buy it for half that price.

21—There ought to be a law against that.
22—Five dollars ought to be a big enough allowance for any boy.
23—The climate of Arizona ought to improve your health.
24—Everyone ought to sleep at least eight hours a day.
25—You ought to brush your clothes before packing them away.
26—You ought to hang your tools up, not leave them lying around.
27—With his constitution he ought to live at least until morning.
28—Every room ought to have windows on at least two sides.
29—There ought to be a guard rail on these stairs.
30—People ought to be allowed to vote after their 18th birthday.
31—You really ought to have an extra blanket handy.
32—Every flesh wound ought to be kept clean.
33—If you observe these precautions there ought to be no danger.
34—A child of that age ought to be able to talk.
35—All motor cars ought to have effective brakes.
36—Six cylinders ought to make a car more flexible than four.
37—You ought to grip the handle more loosely.
38—This road ought to be paved.
39—Everyone ought to have a hobby as well as a main occupation.
40—Two ounces of it ought to be enough to kill a dog.
41—The chimney damper ought to be open while coal is being added to the furnace.
42—Frank would be better but John ought to be able to do the job.
43—The text is all right but the footnotes ought to be in smaller type.
44—On that salary he ought to live very comfortably.
45—We have done many things we ought not to have done.
46—People who are closely related ought not to marry.
47—Verbal tests ought to be superior to motor tests in measuring intelligence.
48—A statistical graph ought to be intelligible without textual explanation.
49—Feeble-minded and insane ought to be prevented from reproducing.
50—An oration ought to begin calmly and work up to a strong climax.

SECTION OF ANTHROPOLOGY

March 25, 1946

Doctor Carl Withers, Brooklyn College, Brooklyn, N. Y.: *The Folklore of a Small Town.*

(This paper will be published in the May issue of TRANSACTIONS.)

SECTIONS OF BIOLOGY AND PSYCHOLOGY

MARCH 1, 1946

Conference on *"Physiological and Psychological Factors in Sex Behavior."*

The Sections of Biology and Psychology held a Conference on "Physiological and Psychological Factors in Sex Behavior." Doctor A. H. Maslow, Brooklyn College, Brooklyn, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

"Animal Endocrines in Relation to Sex Behavior," by W. C. Young, Cedar Crest College, Allentown, Pennsylvania.

"Sex Behavior in Primates," by William Galt, New York, N. Y.

"Sex Behavior in Animals in General," by Frank Beach, American Museum of Natural History, New York, N. Y.

"Normal Sex Behavior in Humans," by Alfred Kinsey, Indiana University, Bloomington, Indiana.

"Aberrant Sex Behavior in Humans," by Morris Hermann, New York University College of Medicine, New York, N. Y.

"Sex and Culture," by Gregory Bateson, Institute for Intercultural Studies, New York, N. Y.

SECTION OF BIOLOGY

March 15 and 16, 1946

Conference on *"The Relation of Diseases in the Lower Animals to Human Welfare."*

The Section of Biology held a Conference on "The Relation of Diseases in the Lower Animals to Human Welfare." Doctor William A. Hagen, New York State Veterinary College, Cornell University, New York, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Friday, March 15

"Introduction to the Conference," by William A. Hagen.

"Rabies," by Harald N. Johnson, International Health Division, Rockefeller Foundation, New York, N. Y.

"Equine Encephalomyelitis," by Raymond A. Kelser, School of Veterinary Medicine, University of Pennsylvania, Philadelphia, Pennsylvania.

"Psittacosis and Ornithosis," by Herald R. Cox, The Lederle Laboratories, Pearl River, N. Y.

"The Relation of Brucellosis to Human Welfare," by I. Forest Huddleson, School of Veterinary Medicine, Michigan State College, East Lansing, Michigan.

"The Prevention of Plague in the Light of Newer Knowledge," by Karl F. Meyer, The Medical Center, University of California, San Francisco, California.

Saturday, March 16

"Animal Tuberculosis and Its Relation to the Disease in Man," by William H. Feldman, The Mayo Foundation, Rochester, Minnesota.

"Anthrax in Animals and its Relationship to the Disease in Man," by C. D. Stein, Bureau of Animal Industry, U. S. Department of Agriculture, Washington, D. C.

"Erysipelothrix Rhusiopathiae Infection in Swine and in Human Beings," by Joseph V. Klauder, Philadelphia, Pennsylvania.

"Animal Parasites Transmissible to Man," by Willard H. Wright, U. S. Public Health Service, National Institute of Health, Bethesda, Maryland.

NEW MEMBERS

Elected March 28, 1946

SUSTAINING MEMBERSHIP

Cathcart, William H., Ph.D., Food Technology. Director of Laboratories, National Bakery Divn., Great Atlantic & Pacific Tea Co., New York, N. Y.
Steinbach, Maxim, M.D. Research Associate in Bacteriology, Columbia University, New York, N. Y.

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Brent, Bernard J., Dr. Eng. (Chem.), Endocrinology, Vitamins, Chemistry, Physics. Scientific Director, Roche-Organon, Inc., Nutley, N. J.
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TRANSACTIONS
of
THE NEW YORK ACADEMY OF SCIENCES

SER. II, VOL. 8 MAY, 1946 No. 7

SECTION OF GEOLOGY AND MINERALOGY

APRIL 1, 1946

DOCTOR ALFRED K. SNELGROVE, Visiting Professor of Geology, Rutgers University, New Brunswick, N. J.: *Some Problems of Newfoundland Geology.* (This lecture was illustrated by lantern slides and motion pictures.)

Geologically, the island of Newfoundland is an extension of the New England-Acadian Division of the Appalachian Highlands. It exhibits areally limited relations with the pre-Cambrian Shield of Canada. The rather complete Paleozoic record is of special interest for its European, as well as for its North American, affinities.

Among the problems discussed by the speaker, to the accompaniment of a motion picture tour of the island, were:

(1) The delimitation of the submarine Ordovician iron ore basin at Wabana, and the possible application of geophysics thereto;

(2) The origin of epithermal deposits of lead, zinc, fluorspar, celestite, etc., in country rocks, of pre-Cambrian or Paleozoic age, on the south and west coasts of the island;

(3) The genetic relationships of base metal deposits in the central mineral belt of the island; and

(4) Paleoclimatic conditions, in respect to the accumulation of evaporites and mineral fuels in this part of Greater Acadia.

The potentially important Huronian iron ore deposits of Newfoundland Labrador, and the magnificent Grand Falls of the Hamilton River, were also depicted and discussed.

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SECTION OF BIOLOGY

APRIL 8, 1946

DOCTOR CHARLES O. WARREN, Assistant Professor of Physiology and Anatomy, Cornell University Medical College, New York, N. Y.: *Tissue Metabolism Studies on Bone Marrow.** (This lecture was illustrated by lantern slides.)

Bone marrow, the site of production of both red and white blood cells, is one of the few tissues of the adult organism that exhibit active growth and multiplication of cells. Metabolic studies of this organ are, consequently, of general interest in connection with problems of normal and abnormal cellular growth. We have employed the Warburg microrespiration technique to investigate certain aspects of the metabolism of bone marrow cells, supplementing chemical and metabolic studies of other investigators.[1-8] It is the purpose of this report to summarize some of the results.

In most of the experiments, the marrow samples were slices or suspensions of rabbit femoral and tibial marrow. Human material was also used; human rib-marrow has been found to show the same general metabolic features as rabbit marrow, and experiments with human leukemic cells will be cited. Details of the experimental techniques may be found in the publications referred to; but it should be pointed out that the duration of the experiments is a matter of a few hours, rather than of days. The tissue survives well during this period; the cells maintain their staining characteristics, and the motile myeloid cells continue their amoeboid activity. The procedure must not, however, be confused with tissue culture methods.

I. THE RELATIONSHIP BETWEEN RESPIRATION, GLYCOLYSIS, AND THE CELLULAR COMPOSITION OF THE MARROW

Bone marrow consists of a mixture of red blood cell precursors (erythroid cells) and white cell precursors (myeloid cells) in various proportions, in addition to a large and variable complement of fat cells.

* This work has been generously supported by grants from the John and Mary R. Markle Foundation.

Metabolic measurements have, accordingly, to deal with this heterogeneity of the experimental material. The variable influence of the fat content is eliminated by expressing the results in terms of fat-free dry weight of tissue, which may be conveniently calculated from nitrogen analyses.[4] In experiments designed to determine the effect of the varying proportions of the erythroid to the myeloid cells,[9] the relative numbers of these cells were varied over a wide range by subjecting the animals to suitable experimental procedures. In this way, it was possible to arrive at estimates of the rates of respiration and glycolysis (lactic acid formation) for the separate myeloid and erythroid components of the marrow. It was found that the erythroid cells are characterized metabolically by a relative predominance of respiratory over glycolytic processes, while the reverse is true of the myeloid cells. The active glycolytic mechanisms in the latter are transmitted to their mature daughter cells, the polymorphonuclear leucocytes,[10] for which this metabolic feature is a physiological asset, permitting them to secure energy for their life processes under the relatively anoxic circumstances to which they are frequently submitted, in their wanderings throughout the organism. On the other hand, it is difficult to understand how the relative inactivity of the glycolytic mechanisms and, hence, a dependence on respiratory processes can be other than a disadvantage to the erythroid cells, which, nevertheless, manage not only to survive under conditions of oxygen want, but even to respond to this situation by increased growth and multiplication. This problem was further investigated in the studies now to be described.

II. The Influence of Oxygen Tension on Marrow Metabolism

It has long been known that exposure to conditions of lowered oxygen tension produces a polycythemia, which is the result of an increase in growth and division of the erythroid cells in the marrow. The metabolic basis of this enhanced growth of the red-cell precursors in the marrow, however, is far from clear. Some interesting experiments of Schultze[6] have shown that the cytochrome oxidase activity of rat bone marrow is increased markedly by exposing the animals, for 48 hours, to lowered oxygen tension. Unfortunately, however, no histological examinations were made, so that it is not possible to determine whether the increased enzyme activity is due to replacement of fat by erythroid tissue, to a higher enzyme content of the erythroid as com-

pared to the myeloid tissue, or to an increased enzyme content of the individual erythroid cells.

We have endeavored to ascertain the total energy available to the erythroid cells under conditions of lowered oxygen tension. When bone marrow is exposed to lowered oxygen tension *in vitro*, respiration is not increased; but, at very low levels of oxygen tension, glycolysis is increased.[11–13] This increase, however, is accompanied by a *decrease* in respiration. Since respiration yields more energy than glycolysis, the total energy available to the cells is less than under conditions of normal oxygen tension. This seems to indicate that the erythroid cells, under conditions of lowered oxygen tension, utilize a larger fraction of their total metabolic energy for synthetic processes, *i.e.* growth. How this is accomplished is not known, but changes in phosphate metabolism may be implicated.[14]

These considerations apply to experiments in which bone marrow is exposed to lowered oxygen tension *in vitro*. When the experiment is performed *in vivo, i.e.*, when the intact animal is subjected to these conditions and the marrow is removed for metabolic studies,[11] there is no evidence that the glycolytic components of enzyme systems are increased as the result of formation of erythroid cells under the influence of lowered oxygen tension. Respiration is higher than in marrows of normal animals, but this is apparently due to the higher proportion of erythroid cells in these marrows. In short, the metabolic studies offer no support for the view that lowered oxygen tension stimulates growth of erythroid cells by acting *directly* on the marrow. This widely-held concept may be true, but, in our opinion, convincing evidence to support it has not been brought forward.

III. Bone Marrow Metabolism in Relation to Tumor Metabolism

In the course of these studies, it became apparent that the respiratory and glycolytic metabolism of the myeloid (but not the erythroid) cells of bone marrow in many respects resembles that considered to be characteristic of malignant tumor cells. Further investigations were then made,[15] and it was found that the myeloid cells fulfilled 8 out of 9 criteria[16] generally accepted as indicative of a tumor type of metabolism. Furthermore, the one criterion upon which they are found to fall into the normal rather than the malignant group of tissues, namely, their relatively high respiratory quotient, is the least dependable char-

acteristic of the nine enumerated, low respiratory quotients being by no means a universal attribute of tumor cells. The resemblance between the respiratory and glycolytic metabolism of normal myeloid and tumor cells is, then, particularly striking. It is especially notable that, although both the erythroid and myeloid cells are actively growing cells in the adult organism, only the myeloid cells show malignant transformation (leukemia). The corresponding malignant disease of the erythroid cells (erythroleukosis) is a rare disease in certain animals, notably in fowl; it is virtually unknown in man.

It would appear, therefore, that the myeloid cells may be predisposed to malignant transformation by reason of the close resemblance of their normal metabolism to that of a tumor type. This observation, in turn, adds weight to the concept that there is, indeed, an important relationship between the respiratory and glycolytic activity of cells and their malignant potentialities.

IV. The Action of Certain Drugs

Potassium arsenite was studied[17] because of its clinical use in the treatment of myeloid leukemia in man. It was found to induce a marked depression in the rate of respiration of normal and leukemic myeloid cells, and a secondary or consequent increase in aerobic glycolysis. However, the drug also affected various other normal tissues in essentially the same way, and given concentrations of the drug depressed the respiration of normal and leukemic cells to virtually the same extent. In order to account for the greater susceptibility of the leukemic cells *in vivo*, one must, therefore, conclude: either, that these malignant cells are less able to withstand a given depression of respiration, or, that the effect on respiration is not the essential feature of the drug action.

Thiouracil, another drug investigated, is now being widely used clinically, in the treatment of hyperthyroidism. Unfortunately, an occasional individual, during a carefully supervised course of treatment, may suddenly develop a serious leucopenia or even a fatal agranulocytosis, due, apparently, to a toxic effect of the drug on the bone marrow. It was, therefore, of interest to determine whether thiouracil influences the metabolism of rabbit bone marrow *in vitro*. It was found[18] that thiouracil does, indeed, have a direct depressant effect on the marrow respiration, and particularly on the myeloid cells of the marrow. It

was necessary to employ high concentrations of the drug (100 mg. %) in order to demonstrate this action, but Williams and his co-workers[19] have found that, in the bone marrow of patients receiving the drug, concentrations of a similar order of magnitude may be reached. Pyridoxine, which has been used clinically to counteract the effect of thiouracil on the marrow, is without demonstrable protective action *in vitro*. It is suggested, however, that these methods might be employed to test new chemotherapeutic agents for possible toxic effects on the marrow; also, to test the action of compounds proposed to protect the marrow from the depressant effects of drugs.

V. The Influence of Serum on Marrow Respiration and Glycolysis

Early in the course of these studies,[20] it was observed that marrow respiration and glycolysis are nearly twice as rapid when serum, rather than the usual Ringer solution, is used as the suspension medium. This action of serum has been noted in the case of several other tissues,[21] but, with marrow, the effect is particularly striking. Contrary to what might be anticipated *a priori*, the serum proteins appear to play a minor role, for protein-free ultrafiltrates of serum are very nearly as effective as serum itself.[22] When the ultrafiltrate is further fractionated, more than half its activity in sustaining marrow respiration is found to be attributable to its content of bicarbonate ions, or to the CO_2 with which the bicarbonate is in equilibrium. Other authors, employing carbon isotopes, have recently demonstrated that metabolic fixation of CO_2 occurs in certain organs of the body, notably in the liver.[23, 24] We find that bicarbonate-CO_2 enhances liver as well as marrow respiration.[22] It is quite possible that, in both sites, the effect on respiration is associated with a metabolic fixation of CO_2.

When freed of bicarbonate-CO_2, serum ultrafiltrate still induces a rate of respiration in marrow and liver that is 25 to 35 per cent higher than in Ringer-glucose solution. The nature of the substances responsible for this residual activity, which may well be preferred substrates for marrow metabolism, is currently under investigation. The evidence, to date, points to their fatty acid nature. There is, however, little information yet available as to what foodstuffs (sugars, amino acids, and fatty acids) are preferentially utilized by the marrow, and it is particularly in this direction that our inquiries are being continued.

LITERATURE CITED

1. **v. Breza, J.**
1926. Arch. exp. Path. & Pharm. **117:** 240.
2. **Felix, K., A. Grassmück, K. Huck, & K. Matzen**
1933. Zeitschr. f. Physiol. Chem. **220:** 137.
3. **Schretzenmaur, A., & H. Bröcheler**
1936. Klin. Wochenschr. **15:** 998.
4. **Orr, J. W., & L. H. Stickland**
1938. Biochem. J. **32:** 567.
5. **Huggins, C., J. McFayden, & E. Wiege**
1940. Anat. Rec. **76:** 309.
6. **Schultze, M. O.**
1941. J. Biol. Chem. **138:** 219.
7. **McCoy, R. H., & M. O. Schultze**
1944. Ibid. **156:** 479.
8. **Scott, E. M., & R. H. McCoy**
1944. Arch. Biochem. **5:** 349.
9. **Warren, C. O.**
1940. Am. J. Physiol. **131:** 176.
10. **McLeod, J., & C. P. Rhoads**
1939. Proc. Soc. Exp. Biol. & Med. **41:** 268.
11. **Warren, C. O.**
1941. Am. J. Physiol. **135:** 249.
12. **Warren, C. O.**
1942. J. Cell. & Compar. Physiol. **19:** 193.
13. **Warren, C. O., & C. E. Carter**
1943. J. Biol. Chem. **150:** 267.
14. **Johnson, M. J.**
1941. Science **94:** 200.
15. **Warren, C. O.**
1943. Cancer Res. **3:** 621.
16. **Burk, D.**
1942. Symposium on Respiratory Enzymes: 235–245. Univ. of Wisconsin Press.
17. **Warren, C. O.**
1943. Am. J. Physiol. **139:** 719.
18. **Warren, C. O.**
1945. Ibid. **145:** 71.
19. **Williams, R. H., G. A. Kay, & B. J. Jandorf**
1944. J. Clin. Invest. **23:** 613.
20. **Warren, C. O.**
1940. Am. J. Physiol. **128:** 455.
21. **Canzanelli, A., & D. Rapport**
1939. Ibid. **127:** 296.
1942. Ibid. **135:** 316.

22. **Warren, C. O.**
1944. J. Biol. Chem. **156**: 559.
23. **Solomon, A. K., B. Vennesland, F. W. Klemperer, J. M. Buchanan, & A. B. Hastings**
1941. J. Biol. Chem. **140**: 171.
24. **Evans, E. A., Jr.**
1942. Science **96**: 25.

SECTION OF PSYCHOLOGY

APRIL 15, 1946

DOCTOR RICHARD H. HOFFMANN, New York, N. Y.: *Psychosomatic Medicine.*

I have chosen the subject of "Psychosomatic Medicine" merely because the word itself is newly minted from dies cut in the time of Hippocrates. In my opinion, the greatest achievement of human thinking, after the impulse of the 'missing link' to strive toward *homo sapiens*, has been the recently proven concept of the essential identity of matter and energy.

The little sister of this Gargantuan discovery has been the slow birth of the idea that body and mind are not separate and discriminate entities abiding in their human host, but that they are kindred expressions of Life answering the laws of Nature.

It has become an axiom among biologists that, in every cell, there exists a primitive consciousness, a sentient protoplasm dedicated to a purpose. That cell developed into an organism of more complicated structure. Gradually there were created, through need, higher centers destined to control and preside over the ever-growing number of impulses and reactions complicating the life of that organism.

Behind all this was the instinct for survival, through which there slowly developed from primitive protoplasm a nervous system that progressed from a ganglian cell to a spinal cord, to a medulla, to a midbrain, and, eventually, to a neopallium. Then came that inexplicable force that we call consciousness and thought, amplified to the point of achieving the knowledge which taught scientists to split the atom. We have also been given the insight to discover that we are a mass of instincts and emotions which, in our higher integration, are subservient to our reason, the distillate of what Dr. Frederick Tilney called the 'master organ of life,' the human brain.

I shall mention only in passing the attempts that have been made to solve the riddle of human consciousness. The approach has been materialistic, mystical, philosophic, behavioristic, biochemical, analytical, and even spiritual. I prefer to look upon the individual, "in conflict with Fortune in men's eyes," from a synthetic standpoint, utilizing every avenue of inquiry, and applying to him the fruits of whatever talent and experience I possess.

Very early in my medical career, after an interneship in one of our most scientifically dedicated hospitals, I found that the rules and methods leading to diagnosis were far from fulfilling the aim of the healer.

A patient of mine, who was a veritable museum of pathology, an encyclopedia of complaint, was going from bad to worse, under my most assiduous ministrations. It was only when I discovered that she planned to marry a pickpocket and might easily have been implicated that I stopped all treatment, and warned her to give up her plans and beware of quicksands upon which she was about to build her home. The patient returned in three months in the very prime of health. I asked her who had cured her, and what had been her malady. She answered, "No one cured me. I broke my engagement. My fiancé is in jail."

From this case, I learned very early in my career that, while I thought I knew a great deal about medicine, I knew very little about the human being. More than thirty years ago, I felt that, in the field of so-called psychiatry, which, at that time, was considered to be the treatment for the insane, I would find the causes of many ills which not only the spirit, but also the flesh, was heir to. There were, however, four horses to the psychiatrist's chariot, driving him to the Revelation—the physical, the chemical, the psychical, and the spiritual. He must not, however, lose his sense of awareness to disease due to infection, trauma, and neoplasm.

Whether a patient complains of physical or mental discomfort, or both; whether his malady be determined as primarily due to tissue change, glandular dysfunction, allergy, emotional instability, habit insult, or inability to fulfill the primary aim of life—to see a goal and progress towards its attainment; we shall find, upon careful inquiry, that, somewhere in the realm of the psyche, there has been an underdevelopment, or an arrest, or a frustration, or a defeat, which has made it impossible for the individual to accommodate himself comfortably to his environment.

"Blessed are those whose blood and judgment are so well comingled that they are not a pipe for Fortune's finger to sound what stop she choose."

We usually see the people who, at some time in life, have been the objects of its processes, rather than subjects who create them. As a result of this conflict, there ensues fatigue. By fatigue, I do not mean

tired muscles. I mean a chemical fatigue in which the necessary fuels that keep the organisms burning adequately are exhausted, spent, or distributed to the wrong ovens. Out of this fatigue is bred apprehension, and continued apprehension fathers distortion.

Two major conflicts beset life: one is something you want and cannot get; the other is something you have and cannot get rid of. In this conflict, the patient falls back from civilized adjustment to primitive mechanisms of thinking and feeling. First came the word. It is over a bridge of words that we lead our patient from dislocated organization back to adequate function. These patients come reminding us of light-struck plates, incapable of taking impressions. They must be resensitized before they can be exposed to their objective lives. That is why the psychiatrist must be an internist and a biologist. If he disregards physiological disproportions and endeavors to constrain his patient to only one method of investigation and therapy, he will find himself like an Icarus, soaring on wings of wax.

The careful psychiatrist has learned that many maladies arise in people who try to fit square pegs into round holes, and insist upon success. Whatever the protest may be, whatever organ may cry out in pain or passion against the injustices of life, it behooves the psychiatrist to ferret out from the history of the patient, from his birth to the time of his appearance, the hurdles that he either had to avoid or knock down in order to keep in the race. Often, it is necessary to educate our patients to an understanding of our interpretation of their problems. I usually draw a pattern in which I symbolize the concept by a triangle, the idea by a square, and the act by a star. I show him that, as he goes from one to another, the stream of energy is directed by currents of reason and emotion. I try to convince him that the adult travels this path led by reason; that the stultified individual—the one who is subadult—follows an infantile pattern dominated by his emotions. I then draw a tripod and label the stanchions "Security, Loyalty, and Love." I try to prove to him that he has superimposed a primitive upon a civilized pattern of existence; that his way to survival is through security; that, without it, neither love, nor loyalty, can play an adequate part in the drama of life. I then explain that loyalty or honesty is a policy and that it is not indigenous to the untrammeled primitive cell, or primitive man! Experience has taught his forefather that, although it was against his nature not to take what he wanted, it was good policy to treat his neighbor as he himself would like to be treated, and thus

avoid the battles born out of revenge and spite. He is made to realize that the history of civilization is the history of the harnessing of our predatory instincts.

Finally, we come to the discussion of love. Just as it has been said that, in the name of the Prince of Peace, more wars have been fought than in the name of Satan and all of his ilk, so, in the name of love, there have been more battles fought among its pensioners than have been waged in the name of all the imps of the cosmos. Here in the sphere of the 'pangs of despis'd love,' we see numberless moods of strife and discord that are born in the spirit of men, reflecting its despondencies in innumerable manifestations of disease.

The Lange-James Theory of the Interpretation of Fear can be applied to any distortion that makes its inroads upon the serenity of human choice. The discord produces a visceral protest, discomfort; the mind becomes aware of the discomfort and reacts to it with fear and apprehension, producing, literally, a new disease.

It would be a wonderful thing if we could take the 'buffets and rewards of Fortune' without disturbing our sympathetic nervous system. Then we could avoid tension and spasm, secretory change, imbalance, and, eventually, organic disease. But the swallowed sigh, having been born in the heart and imprisoned in the stomach, may eventually produce an ulcer. The cure of that ulcer does not lie in local treatment. True enough, with rest and diet and drugs, the ulcer will heal. However, the pain in the heart must be removed to prevent its recurrence.

I may have given stress to the chauvinistic conclusion that, among physicians, the psychiatrist is the portrait-painter. If I were to admit a sub-group, the psychoanalyst, I would call him the caricaturist. The physician, in the ordinary sense, is a photographer. He gets a likeness of his patient by making his necessary scientific exposures and eventually deciding upon the one he likes best, labeling it, and treating the label. The psychiatrist, on the other hand, becomes intimately acquainted with his patient. He sees him as a child, as an adolescent, as an adult. He watches his mood, he sees his relation to time and space, and when he has memorized his personality, he puts him on the platform, and using the brushes and colors of insight and understanding, creates a true portrait which has the likeness not only of the without, but of the within, and reflects the character and disposition of the sitter.

SECTION OF ANTHROPOLOGY

April 22, 1946

Mr. Paul S. Wingert, Department of Fine Arts, Columbia University, New York, N. Y.: *Art and Its Function in the New Hebrides.*

(This lecture was illustrated by lantern slides.)

(No abstract of this paper has been received.)

MR. CARL WITHERS, Brooklyn College, Brooklyn, N. Y.: *The Folklore of a Small Town.**

Although the assigned topic indicates a wider scope, I shall attempt to discuss only the medical folklore of a small midwestern community which I have pseudonymously described elsewhere, in more general anthropological terms. A book-length treatment of its folklore, including the more usual folkloristic categories, is in preparation.

The community will, in this present paper, be called Smalltown. The population of Smalltown is slightly under three hundred, and it is the local trading center for about two hundred farms which lie on a small prairie surrounding the village, and in the nearby hills.

In Smalltown, there are five historical layers of medical and pseudo-medical lore, all of which coexist and have coexisted, in varying ratios, for a long time. They are, in a kind of rough-and-ready historical order of appearance: (1) Early magical practice, including both "witchcraft" and divine healing; (2) an enormous body of "home remedies"; (3) rational or pseudo-rational medicine connected with the recognized medical profession; (4) patent medicines; and (5) a new and recent wave of curing by prayer and other religious techniques.

Old men and women now[1] in their seventies say that, when they were children, "the woods were full of witches." Most witches were women; a few were men, though the word, when applied to a man, usually meant only a "water witch" or dowser, who with a forked stick could locate underground "streams" of water. A few witches had power to harm with ill-luck, sickness, even death. Most were benign, however, serving as "granny women" (midwives), sick-bed nurses, and respected repositories of the lore of home remedies. Women who practiced these arts were not necessarily witches, but most granny women were. Midwifery was a semi-specialized profession.

A granny woman was in particular demand, not only if she had practical command of childbed techniques (some of these were magical: for example, she should know enough to burn cobs under the bed when parturition was difficult), but also if she knew spells to ease pain, could stop blood from flowing by murmuring a Bible verse or magic rhyme, could remove warts with spit or string or words, and

* This address was delivered at the meeting of the Section of Anthropology, March 25, 1946.
[1] "Now" means 1939-40, when fieldwork was done.

knew charms or devices to catch reluctant lovers or to repel undesired ones.

Many people, other than witches, knew and practiced various magical charms, some of which are still used. I imagine that no one any longer tries to capture a sweetheart by stealing a bow off his hat-band, by dropping a toenail clipping into his drink, or by baking a drop of menstrual blood in his cake. The first two are still often laughingly mentioned, though it is "indecent" to speak of the third. However, everyone knows several magical methods of removing warts and tells of occasions when they *worked.* The old-time witch simply knew more magical tricks than the average layman, and her prestige hinged on her store of knowledge in all medical and magical situations. Many witches could also see or communicate with ghosts, interpret dreams, or tell fortunes from cards, tea leaves, coffee grounds, or a bottle of whiskey used as a crystal. Some had second sight. These knew instantly when a distant house or haystack caught fire, and when far-off relatives suffered injury, sickness, or death. All these powers, taken together with a tremendous system of omens, tokens, and signs, related to human health and well-being.

Except for one famous local story, only a few scraps of evil witch-craft are any longer remembered. One way to harm people used to be to leave a "pully bone"—a chicken's wishbone—on the doorstep of an enemy. If he stepped on the bone, he suffered whatever harm had been wished against him. About thirty years ago, the families in a rural neighborhood who used the same spring grew angry, because a man of the neighborhood kept dropping horsehairs into the water, "to poison people." Presumably, the horsehairs would turn into snakes, a notion which probably a third of the people still believe. Many have tested it out by putting a horsehair into a bottle of water for a week or two, and most have found it to be true!

The community's great story of evil magic occurred in 1897, and terminated in a court trial. A man whose farm adjoined that of his two elderly aunts repeatedly found horsehair ringlets in his house, barn, paths, and even pockets. He finally decided that they were "witching" him and prayed to God for guidance. God appeared to him and told him to kill the witches on a night when the moon dipped three times. For three nights he watched the moon and, on the third night, it dipped low three times. He sharpened his corn knife, crept up to his aunts'

house, and cut the women to pieces. At his trial for murder, he showed a pocketful of horsehair ringlets, but they were not accepted as legal evidence and he received a life term in the penitentiary. In a few years, however, he was paroled to relatives in another state, with whom he lived out the rest of his life peacefully.

There are no longer any witches in Smalltown. Elderly women, however, still serve as midwives, usually as neighborly and unpaid assistants to the attending physician. There are also still a few ghost-seers and "dreamers," mostly in the backwoods, and a large body of signs and omens, which some believe in, some half believe in, and others view with mirth or contempt.

Most of what are locally called "the old-home remedies" are passing into disuse, but enough were reported to fill many pages. Here are a few, together with the names of the ailments for which they were, or are, given. Colds are commonly viewed as "something you just have to wear out," yet there are many old remedies for them. One is to eat honey, because it is made out of wild flowers; another, a tea brewed of field balsam, and sweetened. For coughs, drink hot tea made by boiling mullen leaves and hoarhound, or by boiling shredded hickory bark. ("Hickory tea," in another connection, is a "remedy" applied directly with a hickory switch to the backside of a small boy.) For a sore throat, swallow a teaspoonful of sugar saturated with turpentine or kerosene, or blow dry sulphur into the throat through a paper funnel. A preventive gargle for colds is one quart of boiled rainwater containing a teaspoonful of salt, soda, and carbolic acid; this should be used every morning. Another gargle is vinegar, liberally sprinkled with pepper and salt. Some "old-style" farmers and town people swallow skunk oil for colds, though they know that most of their neighbors laugh at this remedy. Skunk oil is also taken internally for "croup," dropped into the ear to stop earache, rubbed on the chest for a chest cold, and applied externally to any part of the body beneath which there is a pain or "information" (inflammation).

To cure colic in her seven-day-old baby, one young mother administered sulphur soaked with the juice of a baked onion. An elderly neighbor had suggested, instead, two teaspoonfuls of boiled catnip juice with sugar. Calamus root tea is occasionally given for "cramp colic"; the boiled juice of rhubarb root, for "stomach cramps"; spice tea, for

"the stomach ache." For "a rising in the bowels,"[2] one old woman recommended a hop-tea poultice, made of the boiled cones. "A fine remedy for the flux or summer complaint," she said, "is mules'-tail tea." Mules'-tail is the native name of a plant. An old man said, "We don't *never* go to a doctor for summer complaint. We just bile witch hazel bark or the sticks, either one, and drink the juice. It's a *cure* for people or hogs that's got the scours." He cures styes by applying castor oil, though his father's remedy was "weak terbacker ooze"; "the seven year eetch," by boiling polk root and rubbing it on. This remedy he described as "from a way back" and severe—"It'll raise up big welts . . . but hit'll sure cure the eetch."

An old couple living in town have cured hives with (1) saffron tea, (2) an onion bandage, or (3) a peppermint poultice. The old man has cured himself three times of "blood boils" by drinking burdock-root tea. With this treatment, in 1901, he cured himself of a series of twenty-five boils on his neck. His wife has cured shingles with soda and with catnip tea, applied to the rash; but the best remedy, she said, is to "cut off a black cat's tail—or a black chicken's tail will do—and let it bleed over the sores." She cured her husband, using a black chicken, in 1931.

She, at least, does not think of this treatment as magical, but just as an old-time home remedy that works. In her childhood, "everybody" took sulphur and molasses, each spring, to thin the blood. The practice was to take a dose on three successive mornings, miss three days, and so on, until nine doses were taken. In her childhood, also, she said, though nobody else mentioned this, scarification was widely

[2] There are scores of curious local names for illnesses, for example: "risings" in the head, stomach, leg, or any other member; "asthmy" of the windpipe or stomach; "information" of almost any part of the body, including internal organs; "congestions"; "nerve trouble"; "female complaint"; kidney, liver, stomach, or heart "trouble"; "slow" or "quick" (sometimes called "galloping") "consumption"; etc., etc.

Some diseases are considered to be "imaginary" or to rest only "in the mind." Most sufferers are said to be women. The main causes are "a nervous disposition" or "brooding" (perhaps over a headstrong child or sinful husband or a recent death in the family) or from "thinking about one's self too much." Such a disease may have or lack organic symptoms. The "worst" mental disease is, of course, insanity. There are two kinds of insanity, the "harmless" kind and the "violent" kind, and the former gains much more sympathy for its victim than the latter. The wife of a leading citizen was insane while I was in Smalltown. She was not considered really dangerous, but her outbursts of extremely frank talk embarrassed people. Her illness was almost universally attributed to her having washed clothes while menstruating, yet a good many people thought that she was no crazier than she wanted to be. I rather often heard insanity described as voluntary. "Brooding over religion" is probably the commonest cause of insanity in Smalltown; a number of past suicides were by people who "went crazy over religion." The insanity of a middle-aged man now "in the asylum" was explained as follows: When he was a boy and his mother wanted to punish him, she used to seize him by the heels and dangle him over a well. "She done that a lot when he wouldn't mind her, and that always did seem to bother him a little and prey on his mind." To have kinfolks in the asylum is somewhat disgraceful, and to have "insane blood in the family" is a fearful thing.

practiced. "My older sisters," she said, "didn't have an inch of skin on their shoulders that wasn't welts." To thin the blood in spring, or for agues, bitters were taken until recently—"a big swaller every morning before breakfast." Bitters were made of "sour sassaprilly," "yellow pecoon," ginseng, wild cherry bark, and snakeroot, boiled to a tea to which whiskey was added. Sassafras tea is still taken to thin the blood.

A farm boy in high school mentioned with ridicule all of the following remedies and said they were "still in use": putting a spider web on a wound to stop bleeding; applying salt to a cut to heal it; smoking coffee for toothache; a quid of tobacco on an aching tooth; a tobacco quid on a baby's stomach to cure "worms"; pumpkin seed tea for "stomach worms"; salt and vinegar poultices on the back for kidney trouble; gunpowder and cream ointment for ringworm; canning acid and pine tar salve for eczema; nicotine from a pipe for eczema; golden seal for sores in the mouth; applications of cow dung tied in a white flour sack to mump-swollen testes to draw out the swelling (cow dung is also sometimes used as a face bleach); application of a mixture of coal tar, turpentine, and camphor to the right side to cure appendicitis; and carrying a buckeye or a lump of sulphur in the pocket to cure or prevent rheumatism. In a forthcoming book called *Ozark Superstitions*, Vance Randolph mentions a respected Ozark physician who carries a buckeye for rheumatism, on the basis that it *can't* do any harm and *might* help. Patent medicine is, for the most part, a commercial development from the recognized medical profession, but, since its main use has always been to by-pass doctors and their fees, it is best treated in connection with home remedies. The main difference, in local eyes, is that home-prepared remedies are made from materials that "*we* know," while patent medicines are prepared from formulae that "city doctors and manufacturers know."

Patent medicine came into great favor at least forty to fifty years ago, although this seems also to have been the period of greatest rivalry between all methods of curing; through witches (specialists in both magical formulae and home remedies); through a far wider parental knowledge of home remedies than at present; through patent medicines of various kinds; and through regular physicians. There were over a dozen physicians in the county in 1900. In 1940, there were only three, only one of them active. Whether sickness has diminished during the

same period, I do not know. Some oldsters say, "There wasn't no sickness around when I was a child." Others say, "In them days ever'body was *down* half the time." Medical records were wholly lacking until recent years and are still inadequate. Malaria does seem to have been very prevalent until recent years, and typhoid fever a scourge every summer. Undoubtedly, there were also more frequent epidemics of such diseases as whooping cough, measles, mumps, scarlet fever, smallpox, and diphtheria.

A type of patent medicine, no longer sold, was the universal nostrum. A dozen curealls were advertised in the county forty years ago, as curing anything from piles to "consumption." Other patent medicines were more modestly claimed to cure only piles *or* consumption, or female complaints, or nervous disorders, or rheumatism and lumbago, or diseases of the stomach, kidneys, liver, and bowels. Numerous patented tonics, salves, liniments, and physic pills were on the market. At picnics and carnivals, barkers sold the panacea "bear grease" and various potions and lotions in bottles, powders, salves, and pills. The labels on many packaged remedies bore the names of doctors and thus carried the aura of the medical profession. Perhaps some of these doctors were imaginary. Some were, of course, authentic medical doctors who were willing to lend their name to products which promised to harvest dollars from the sick and gullible, and the public had even less legal protection from charlatans than it has today.

In the average Smalltown kitchen, there is a shelf on which sits the family's staple supply of "boughten" home remedies.[3] Not all of these are patent medicines. They generally include, minimally, palliatives like aspirin and vaseline; a widely advertised mentholated salve (used in various ways for colds); turpentine (for liniment and antiseptic); and one or more varieties of patented physic pills. Most families have also a supply of epsom salts, castor oil, and calomel. Regular bowel movements are highly valued, and both children and adults are dosed with physic at bedtime after any day of "irregularity." Probably most old people take a physic daily, sometimes five or six times the dosage recommended on the package. Whatever other patent medicines are bought are usually purchased for specific or chronic ailments,

[3] A clock generally sits there too, and often, on nails beneath the shelf, hang a number of almanacs, which are distributed free through local merchants by various patent medicine firms. In addition to advertising matter, these booklets include a calendar, detailed weather forecasts for the year, and information about the zodiacal signs and moon changes, on which a great deal of local agricultural magic is based.

like stomach trouble, nervousness, fatigue, etc. The customer decides, after comparing symptoms with neighbors, or after consultation with a physician, what his illness is, hears radio advertising, reads ads in patent medicine almanacs and newspapers, examines labels on packages in the drugstore, consults druggists and friends, and shops around, trying out this or that packaged remedy, until he finds something that "seems to help," or until his ailment abates. The supplies on the medicine shelf are the present-day basic body of "home remedies," having largely supplanted the older home remedies described earlier, which were in addition homemade, mainly of leaves, roots, and barks found in the local woods. They are usually administered without the advice of a doctor. Doctors and other health officers of the region in general rather weakly sanction their use, though condemning their "abuse."

When children or adults become ill, home diagnosis and home treatment are generally tried first. Home diagnosis of the child, the husband, or of herself, is usually done by the wife, though some men are also "good in sickness," which means good in diagnosis, treatment, and "care." The five primary diagnostic points are these: (1) Testing the sick person's brow with the back of the hand for fever. If the brow feels much hotter than the hand, the patient is "feverish." (2) Examining the tongue to see if it is coated. (3) Asking, "When did your bowels move last?" (4) Asking, "Does your head ache?" And (5) asking, "Do you hurt anywhere else?" A child, who may have been "piecing" from the cupboard or eating wild or tame fruits gathered outdoors, is also asked, "What did you eat?" Except for colds and children's epidemics, most illness is at first diagnosed as due either to constipation or an upset stomach, and a physic is taken or prescribed. The "severe" physics, epsom salts, castor oil, and calomel, are dreaded and hated by children, the first two for their evil taste, and calomel because children cannot eat anything sweet or sour for some time after taking it or they will be "salivated"; that is, their teeth will come out in a horrible manner which will leave gaping holes in their cheeks. These physics are administered to children partly as medicine, partly as punishment, for eating things they knew they shouldn't, like green apples or too many wild cherries or raw turnips or sweets.

An old country proverb goes: "When home remedies fail, try a doctor." There is one aged physician in the community, whose tech-

niques are presumably similar to those of other rural practitioners of his generation—perhaps better, since his son, with whom he is on extremely intimate terms, is a respected physician in the regional metropolis, a city of 60,000, sixty-five miles away. There is also a young osteopath in another town in the county.[4] There seems to be no great local recognition of the differences between the two types of skill and training. People select between the two on the basis of past experience, the comparative age and "modernity" of the two men, and out of a whole welter of hearsay and comparisons of cases of "kill" and of "cure."

Other general practitioners are consulted in nearby county seat towns, but since home call rates are based on mileage, they are too expensive for ordinary home calls. Beyond the immediate area is the regional metropolis, where people go to consult specialists, for hospitalization, for major operations, and for some minor ones. Emergency cases, for example, of acute appendicitis, or what is diagnosed as such, are rushed there in the local undertaker's ambulance.

There are also a few "radio doctors" and "mail order doctors" who advertise in the press, whom some people patronize. The local jeweler and tinker has "got more good" from one of these, with whom he is in correspondence, than from all the "regular doctors." He claims to have spent, in earlier years, thousands of dollars with physicians, whom he calls "butchers and thieves." His mail order doctor denounces all drugs, and prescribes foods, particularly melons, prunes, bran, and mineral oil as medicines. For the notorious "goat gland doctor" Brinkley, whose broadcasts from Kansas were terminated by law, there was about as much sympathy as condemnation, and some people still listened to him broadcast from Mexico. A cancer hospital in the state, which "cures without operations," carries on local propaganda, and it is well thought of. Some people "swear by" chiropractors, though there is no chiropractor nearby.

Beyond all these regional services is the world of "big city doctors," which everybody has heard of, though none has patronized. People have heard of doctors who "charge you $100, just to see if your tongue is coated." Some have read of psychiatrists and other "mind

[4] Since my study was made, in 1939–40, the old doctor has retired and been supplanted by another osteopath, and a third osteopath has come to the county seat A large college of osteopathy in the state is pursuing a very successful program of training young men and encouraging them to locate in the numerous small towns of the state, which no longer attract M D 's

doctors." One young man in the community, otherwise highly respected, is a peeper. He has been known to peep through the bedroom window of two girl cousins. Since this is a family matter, he has gotten into no serious trouble, but a neighbor took the locally unusual view that this young man was "sick and couldn't help his peeping"—and really should consult "one of them city sakitrusts."

Another medical resource that should be mentioned is the District Health Office, fifty miles away and covering five counties. This is a partly fact-finding, partly educational and steering service. In fact-finding, according to a District Nurse, "We have just barely scratched the surface in this county." She hazarded the very tentative and undoubtedly exaggerated guess that *all* the children were rachitic and that ten per cent were tubercular. Her office had no information on whether there was any trachoma or undulant fever in the county, though it is in a region where both have a rather high incidence. As a reform service, the District Health Office works through such agencies as the county relief office, the county agricultural agent, the home economics demonstration agent, and vocational agriculture teachers. No citizen ever visits the office directly, though patients unable to pay are sometimes routed by it to clinics and to state hospitals.

The one dentist in the county provides probably more than half of the professional dental care received by the county's six thousand citizens. The rest that is done, is done by dentists in adjoining counties. A small group of adults try to keep in repair their own teeth and their children's "second" teeth. Milk teeth are not worth spending money on. The rest of the community gets no dental care whatever, except extractions, and usually, at last, false teeth, either fitted by a nearby dentist or bought by mail.[5] Nowadays, most extractions are made by a dentist, but gruesome tales are told of the days when people visited farmer-blacksmiths who pulled teeth with their forge tongs, sometimes just as "an accommodation," sometimes for a small fee. Some adults have been known to pull their own teeth. Those who do not visit a dentist for fillings buy a good many commercial toothache-killers to stuff into the cavities. One man told me that, for a particularly severe toothache, he plunged a red-hot piece of baling wire deep into the

[5] Among people with artificial teeth, there is much talk about how they fit and the "satisfaction" they give or don't give. It was considered odd that a newcomer, the wife of a government employee, never mentioned "her teeth," but seemed embarrassed when told how pretty they were. Old people without teeth, or with just a few teeth, enjoy a good deal of pleasant conversation on such topics as proficiency at "gumming it" or whether they still have two or more teeth that "hit."

cavity. "It hurt like Sam Hill," he said, "but that was the end of my toothache." Extractions, it might be added, seldom come at the same time in the history of a tooth when a city doctor would extract it.

People, in general, think there is not much to be done about dental decay. Either you have good teeth or you don't. Many think that, if you start dental repairs, the dental work itself, particularly cleaning, which, they say, takes off all the enamel, stimulates further decay. They point out that this is to the dentist's financial advantage. Some oldsters say that people used to have better teeth than folks have nowadays, attributing this fact to tobacco chewing, clay pipe smoking, the prevalence of home-grown foods, and many other factors. Some modern ideas are coming in that brushing the teeth prevents decay; that oranges prevent decay; that milk prevents decay.

It is told that, in the old days, a good many old people grew a complete third set of teeth, long after their permanent teeth were gone. At least ten long-since-dead oldtimers are supposed to have done so. A related story is that many old people in the past, after years of fading eyesight, received their "second sight," and for the rest of their lives were able to read their newspapers and Bible (if they knew how to read) or bead their rifles on a squirrel, as in their days of youth.

Before going further into the attitudes toward medical facilities and the extent of their utilization, it may be well to describe the wave of divine healing which has swept one sector of the county in recent years. It is connected with the Holiness Church. No completely faithful member of this organization puts any faith in doctors as curers of disease; though it is all right to use a doctor as a bonesetter or to sew up a cut. Even this should not be *necessary,* however, if your faith is strong enough in God, because He can heal anything. In this whole cult, the widespread popularity of Christian Science, on a relatively high class level, seems to have encouraged and sanctioned the rebirth and expansion of a lower-level and older series of religious superstitions. The Holiness taboo against patent medicines is also rather great, but against old-time home-made remedies there is very little taboo. Apparently, in Holiness theory, these are not "medicines"; they pre-date doctors and drugs, and share none of their evil. An old man, whose wife was a leader in the Holiness congregation, described many old-home remedies, testifying to their efficacy, and he also narrated many cases of cure from prayer. Here are a few: His wife was

down with ulcers; "She couldn't raise herself up and hadn't et for several days; the neighbors said, 'Git a doctor—she cain't live!' " Brother Miller (the preacher) came and prayed over her. She arose fully recovered from her sickbed and ate a big supper. "Now, when you see anything with your own eyes you believe it," said the old man; "I believe in a doctor, too, and I asked the doctor and he said, 'Prayin' cain't do no harm!' " The same man's daughter-in-law got a coffee bean in her windpipe. "The doctor said, 'Operate!' But Brother Miller and others came and prayed and she coughed it right up." At a camp meeting, there was a boy with "paralzy" in one leg. "He was all drawed up—hadn't walked for years. They prayed for him and he straightened right up and walked. Now nobody couldn't doubt that, surely!" A cow hooked the old man's neighbor, Molly, in her arm, tearing the "leaders." The preacher and others gathered for prayer, and Molly suffered no more. The old man himself received a "bad horse-kickin'," a few years back. His life was despaired of, but prayer cured him quickly. " 'Course . . . the Lord don't intend to heal 'em all," said he. "It ain't His will."

In fact, the main pillar of the Holiness Church, a widow, had just died of cancer, though committees from the church met nightly during many months to pray over her, and preachers flocked in from adjoining counties to help. For weeks, in an affiliated church called Pisgah, in far away California, perpetual prayers were said, and, as a ritualistic action, airmail letters were kept constantly en route between Pisgah and Smalltown. I was unable to learn their contents. An anointed handkerchief was sent from Pisgah to lay on her cancerous breast. Non-Holiness people criticized and ridiculed these proceedings, and some said, "She ought to be forced to have a doctor," but no physician saw her. Her death did not weaken her co-religionists' faith in prayer, but was taken as a manifestation of God's will. For her, it was interpreted as a release from earthly suffering into eternal bliss. Other cases were cited, in which it was God's will to cure even cancer. While most religious people in the community believe in the efficacy of prayer in sickness, in encouraging good crops or financial success, and for other matters, most non-Holiness church people consider that its proper medical use is as an adjunct to medication by traditional remedies or by a physician. A man who repeatedly testified in the Methodist Church how God cured his rheumatism at the same time He converted

him from "infidelism and a life-long quest of sin," is officially approved by the preacher, but laughed at privately by many members.

The local physician is usually consulted or called in only in cases of sudden severe illness or injury, or when chronic symptoms of one sort or another have not "worn themselves out" or disappeared after home medication. A deep ax-cut or knife-cut or a shotgun wound is nearly always taken to the doctor for dressing; so is a hand mangled in a piece of farm machinery. Minor cuts or bruises are, of course, handled at home, with or without antiseptic, commercial liniment, poultice, or bandage. If a rusty nail is run into the foot, it may be pulled out with nothing further done; the wound may be soaked in turpentine, iodine, or other antiseptic, at home, and bandaged; or, the patient may be taken to the doctor for treatment and even for anti-tetanus shots.

A broken arm or leg is always set by the doctor. A broken rib or finger is very often allowed to heal of itself. People feel little concern if a broken finger heals crooked or stiff. Almost anyone bitten by a rattler or copperhead would get anti-venom injections from the doctor, though dozens of traditional snakebite remedies are known. These include: liberal dosage with whiskey; soda and onion juice squeezed on the wound; and plunging the bitten finger or toe into the body of a live setting hen, to remain there until the hen dies. There is a madstone in the county. It came from the stomach of a white deer, and old people say "Nobody ever went mad who used that madstone." However, nowadays, anyone bitten by a dog proved to be rabid, takes the Pasteur treatment. The rabidity of the dog is determined by sending its head to state health officers for examination. *Suspicion* regarding rabidity is aroused if the dog is "a stranger," or if he froths at the mouth, or otherwise behaves oddly.

Native attitudes toward recognized physicians and standard medical procedures are a curious hodgepodge of old and new, of suspicious resentment and dependence. Part of this resentment reflects an attitude of suspicion toward science and all its works, including evolution. Part of it comes, of course, from the fact that medicine suggests disease, pain, and ultimate death—subjects which are not very pleasant to think about. People utter proverbial generalizations, like "Doctors don't know anything!" or "With doctors, it's all guesswork." Many natives speak of doctors as callous butchers or, like lawyers, as interested in prolonging a case for what they can get out of it. The finan-

cial motives of most doctors are considered questionable, and sometimes very strange rationalizations are offered. I heard one man deny the existence of "rabbit fever." He had skinned and dressed rabbits all his life without catching anything, and he said all the talk about rabbit fever resulted from a league between the doctors and meat-dealers, to prevent people from eating meat which costs nothing. The highest tribute to a doctor is to say, "I have *faith* in him." The word "faith" here carries a kind of overtone of the sacred, as in the phrases, "faith in God" or "faith in Salvation."

Vaccines and serums of all kinds are viewed with suspicion. Few children are immunized against smallpox, and any movement to introduce compulsory immunization of school children is cried down. Many people say, "The vaccination is worse—and kills more people—than the disease." Others view vaccination as valueless, because its theory, as the theory is locally understood, seems foolish and unreasonable. "To stick dead bugs into you to keep live ones from working on you—now you *know* that's silly!" Still others consider vaccination to be sinful, nor are they all prayer-curers. Yet, if a child has diphtheria, parental anxiety is so great that most parents welcome injections of the serum (a few will refuse!) and speak of its discovery as a great thing.

The urban prestige value of operations has only begun to penetrate Smalltown. A few women are said to "want operations" and to boast of them; and, oddly, people seem rather proud of tonsilectomies for their children. However, in general, surgery is viewed with great fear and horror, and as often useless. Appendectomies, in particular, are thought of as a surgical 'racket.' There are stories of people threatened by a doctor with death, unless they submitted to an appendectomy, but who just took a physic, got well, and stayed well. Others went through the operation only to have the symptoms recur later. In a new operation, the appendix was found intact. The widespread story is told (with local application) that, in one case, a pair of scissors and a wad of gauze were found sewn in beside it. People seem pleased to narrate and to believe such stories.

Hospitals, though beginning to be patronized fairly extensively, are often spoken of as places "where more people come out dead than come out alive."

Physicians themselves help perpetuate this piece of folklore, though the point they intend to make is somewhat different from that

which native critics intend to make. Hospitals, however, are dreaded excessively, and few people go there, except on very serious business. Only one or two Smalltown babies a year are born in hospitals, and it would be almost unthinkable to enter a hospital for diagnosis only. An odd bit of local history or folklore, which many informants mentioned, illustrates how very new cultural traits are often integrated with very old patterns. About fifteen years ago, a man died who, for twenty years or more, had earned a few dollars each year by peddling "The Farmers' Almanac" about the county. He was also an expert in moon signs and zodiacal signs (then even more important than today, in matters of planting, harvesting, girdling trees, building rail fences, shingling houses, castrating livestock, and weaning young animals or babies). When physicians began to prescribe operations, he gained considerable local fame by advising patients whether the sign was wrong or right to have their tonsils or appendix cut out. People say, "He never missed it." If he said the sign was *wrong*, and they went ahead, they always died, or at least had a very hard time of it; but, if he said the sign was *right*, then they knew there would be no danger.

Explanations of health or disease run a very wide gamut. Malaria, once universal, is now practically non-existent, but people still talk about it. What caused it? And what caused it to disappear? Most people accept the fact that infected mosquitoes transmit malaria; but, since there are still mosquitoes, it is hard to understand why malaria slackened, unless it took a greater *number* of bites than people get nowadays to catch malaria. It is said that everybody took a lot of quinine during a good many years, but the *mosquitoes* didn't take any quinine! Some people cling to the early theory that dampness, not mosquitoes, caused the malaria of early days. There were more trees in the river bottoms, and the air was damper. Besides, it *rained* more then, because the soil (now 52 per cent eroded!) produced better crops than it will grow today. Others say that a mosquito may cause malaria, but so may a chigger, a tick, or any other insect that bites. Some people say that mosquitoes must have caused typhoid, once prevalent (people say) every summer. The argument runs: Mosquitoes caused the malaria; typhoid and malaria disappeared at about the same time; therefore, mosquitoes caused the typhoid. Indeed, it is difficult to convince people of any completely credible connection between typhoid and lack of sanitation, because some families drink water out of green-scummed springs, or out of improperly walled and covered wells, and

remain ostensibly hale and hearty. The hotel well stands twenty-five feet from the hotel privy, and slightly downhill from it. I asked the aged proprietor if he had ever had his water tested. "Yes," he said, "a man came through here from the capital and tested it, and he said it was ninety-nine per cent pure. He said that, by rights, I ought to concrete it down about six feet from the top, but I didn't think that was necessary for just that one per cent. *I* never got sick and I never knowed nobody to get sick from drinkin' this water."

The whole problem of reform in sanitation presents many difficulties. People, in general, believe in protection against "germs," though some view germs as "just a superstition of city folks." Separate towels at home, or separate drinking cups at school, are hard to accept, because it is unsociable not to share a towel, and insulting not to want to drink after somebody else. A system of traditional etiquette and courtesy covers all such matters. A district nurse told of families in an adjoining county who were persuaded to construct sanitary privies, but who abandoned them, because "the children like to go together." In certain very old techniques, like dairying, as a matter of fact, some of the old-fashioned people scald and sun their milk vessels carefully and attain a higher level of sanitation than more modern neighbors with cream separators.

Under the stimulus of the agricultural experts, reform has gone further in farming techniques than in medical practice. The current trend is, on the whole, toward cementing and covering wells, clean dairying, and clean poultry yards. Agricultural experts have progressed rather far in introducing techniques which bring profits, even when these involve sanitation.[6] More of the actual reforms in public health, also, have come from them than from medical practitioners, in-

[6] An account of veterinary lore and practice would be interesting, if there were space. Local specialized "curers" are in conflict with the agricultural experts (the nearest veterinarian is too far away to patronize at reasonable cost), who are winning the battle. The chief symbols of resistance to veterinary science are two diseases of cows, "hollow horn" and "losing the cud," which the experts ridicule and the traditionalists angrily defend and treat. Vaccination of animals meets with less resistance than vaccination of children, perhaps, partly, because the animals "have no souls." The main reason is, of course, that animal vaccination is part of a "great trait complex," scientific agriculture, which, for the most part, is being intelligently propagandized. The main traditional symbol of resistance to the whole trait complex of scientific agriculture is the moon. The moon's waning and waxing, together with the zodiacal signs, formerly governed the timing and success of most agricultural pursuits. The zodiacal lore has almost vanished, but fully half the farmers follow the "moon signs" (the "light" and the "dark"), in some or many activities; even more people "believe in the moon" than work by it. New agricultural experts are always quizzed as to "what they think about the moon," and the tactful ones answer evasively. One clever vocational agriculture teacher used to answer the query by saying, "My father always planted his field crops by the moon, but my mother planted her garden any time she thought the soil and weather were ready. And his crops never seemed to do any better than hers." This remark was accepted as perfectly satisfactory.

cluding the regional public health officers. These reforms enter through vocational agriculture classes in the high schools, and through the home economics clubs organized by the county agent, which study nutrition, child care, and sanitation. Active suggestions also come in through home, health, and diet pages in newspapers and magazines. In these, health and prestige values are built up simultaneously. Advertising pages (and radio advertisements), which appeal to people, for prestige and for health, to eat various proprietary foods, probably do more harm than good, for they stimulate people to think of health in terms of single foods. The national enthusiasm for "proper diet" introduces and arouses a new folklore, including many strange and garbled notions: that if people just ate raw foods they would be healthy; that they would be healthy if they ate less meat, or *nothing* but vegetables; that all they need is plenty of vitamins (vitamins tend to be animized, by those who "believe in" them, as little, live, health-giving germs that sit on the edges of cabbage leaves); that oranges, or prunes, or bran, or milk, are the single *sine qua non* of health. I have heard it said that "hog meat causes all the cancers in this country," and that anything out of a tin can will make people sick.

The problem with all folklore is what to make of it. There has been endless collection of old remedies, superstitions, beliefs, legends, riddles, rhymes, proverbs, games, folksongs. Aside from the labors of a number of part-time workers in the field of primitive folklore, the concentration of effort has been on accumulating texts and variants, and on establishing distributions of themes and items. This antiquarian pursuit no longer seems very fruitful or illuminating. Better techniques should be devised to explain the use and meaning of folklore in the social life of the group from which it is collected.

There is, perhaps, one very useful method of dealing with the kind of material presented in this paper. The mixture of ancient and modern fact and fancy which I have called the medical folklore of Smalltown resembles every other aspect of the community life, as I have described it elsewhere. Smalltown is an oldtime hillbilly farming community, in the throes of adjustment to urban influence entering by every device of modern communication: radio, newspapers and magazines, travel, and direct governmental intervention through numerous agencies and facilities. Under the impact of the new, old attitudes are

being abandoned, or reinterpreted, or, as a form of protest, reinforced. This hodgepodge of medical lore well illustrates the nature of the conflict, and of the resultant disorientation. The conflict is, in the main, along class lines and between the generations. Though class is locally denied, there is an upper class of more modern "prairie" farmers and a lower class of "hill" farmers.

New remedies, new foods, new rationalizations regarding medical care and food, disturb the old pattern of "felt security," as the young people, the "better class" people, the new federal employees (the reformers!) condemn, ridicule, and try by a frontal attack to change the ancient way of life. Some people accept the new willingly, but a sense of inferiority and loss comes over most people when they feel disrespect directed toward what they were taught by tradition. Some attack the new with venom and all their skills of rationalization. The whole social structure suffers; the young lose respect for the old; the old condemn the young; family and neighborhood solidarity weaken. The old system of treating the ill involved much cooperation on a neighborly pattern: exchanging remedies, sitting up with the sick, sending food, prayer meetings, etc. The new method weakens the drama of sickness—and even of death.

There might be still another method of dealing usefully with material of this type. Most of the data given were not collected as folklore, but entered as a natural part of conversation into the field notes of a research project which had another purpose. If such materials were collected intensively, rather than incidentally, and by people trained to understand their medical basis, and if they were analyzed fully in terms of the problems of cultural change, they should be useful to reformers, including public health officers. The public health reports available on Smalltown were only statistical, and too inadequate, statistically, to be of much use to an anthropologist. Vital statistics were given which local experience showed to be incorrect. The main facilities, doctors, hospitals, district offices, etc., were listed. Professional pride was expressed in increasing personnel and facilities, and in the increasing use of all these. Yet, there seemed to be too little knowledge of who utilized them, who did not, and why. In speaking with local physicians and health officers (except for an occasional district nurse), I heard little sympathy for, but much contempt and ridicule of,

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SECTION OF BIOLOGY

April 19 and 20, 1946

Conference on *"Some Aspects of Red Cell Production and Destruction."*

The Section of Biology held a Conference on "Some Aspects of Red Cell Production and Destruction." Doctor Eric Ponder was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Friday, April 19

Morning Session. Chairman, Eric Ponder, The Nassau Hospital, Mineola, N. Y.

Introduction to the Conference, by the Conference Chairman.

"Red Cell Cytochemistry and Architecture," by Eric Ponder.

"The Endocrine System and Hemopoiesis," by Albert S. Gordon and Harry A. Charipper, Department of Biology, Washington Square College of Arts and Science, New York University, New York, N. Y.

Afternoon Session. Chairman, L. Michaelis, The Rockefeller Institute for Medical Research, New York, N. Y.

"Hemoglobin and Red Cell Production in Experimental Hemorrhage Anemia," by F. S. Robscheit-Robbins, The University of Rochester, School of Medicine and Dentistry, Rochester, N. Y.

"The Iron and Porphyrin Metabolism in Relation to the Red Blood Cell," by S. Granick, The Rockefeller Institute for Medical Research, New York, N. Y.

Saturday, April 20

Chairman, W. B. Castle, Harvard Medical School, Boston, Mass.

"Etiological Considerations in Macrocytic Anemia," by W. B. Castle.

"The Hemolytic Anemias," by William Dameshek, Tufts College Medical School, Boston, Mass.

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Elected April 25, 1946

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SECTION OF GEOLOGY AND MINERALOGY

MAY 6, 1946

DOCTOR GEORGE GAYLORD SIMPSON, Chairman, Department of Geology and Paleontology, The American Museum of Natural History; and Professor of Vertebrate Paleontology, Columbia University, New York, N. Y.: *Tertiary Land Bridges.* (This lecture was illustrated by lantern slides.)

An early discovery of paleontology was that animals now confined to one part of the globe may occur as fossils in different and distant regions. In some cases, such as that of the elephants and their allies, it would now be impossible for the animals to migrate between the regions where they currently occur, and regions where they were formerly abundant. It was necessary to conclude that regions now separated by the oceans have been connected by land, at various times in the geological past. It has also been found that continents now connected by land must have been separated during some geological epochs.

The problems involved in these varying land connections and separations are among the borderland studies that overlap two, or more, scientific disciplines. Solutions of these problems are essential to both geologists and zoologists, and such solutions can only be reached by combined study, from both the geological and the zoological point of view. Some zoologists have concluded that the recognition of related, or supposedly related, animals in two widely separated regions justifies the postulation of a direct land route between those regions, even though this may be in reckless violation of geological principles and

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probabilities. Some geologists, particularly a few of the advocates of continental drift, have postulated continental connections of a sort and at times and places indicated as practically impossible by the zoological data.

There have also been numerous sober and careful studies of intercontinental connections, or their absence, but even these have necessarily been incomplete and inconclusive. My efforts to evaluate these studies, and to contribute to making them more precise, have been based primarily on the re-examination of faunal relationships that are known, beyond reasonable doubt, to have involved the rise and fall of intercontinental land bridges. The simplest and clearest case available is the relationship between North and South American land faunas during the Cenozoic. This has been analyzed elsewhere, in some detail.[1] Among other things, there emerge, from this and similar studies, certain faunal criteria for the existence, position, and nature of land bridges, especially that postulation of a given bridge requires evidence of:

1. Exchange of varied types of land life, not only of one kind of animal.

2. Faunal interchange in both directions.

3. Interchange involving or maintaining ecologically balanced faunas.

When these conditions are not met by a postulated land bridge, the alternatives to be considered are:

1. Indirect migration, *e.g.*, *via* the well-established North American-South American and North American-Eurasian bridges, rather than by a direct trans-Pacific Asian- (or Australian-) South American bridge.

2. Migration without a continuous bridge, by what I have called a sweepstakes route, *e. g.*, between Eurasia and Australia, or the American continents and the West Indies.

More recently, I have been working on the application and extension of these and other principles, with regard to Eurasian-North American Cenozoic faunal and continental relationships, a much more com-

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[1] See, especially, **Simpson, G. G.**, Mammals and land bridges. J. Washington Acad. Sci. **30**: 137–163. 1940; and **Simpson, G. G.**, Turtles and the origin of the fauna of Latin America. Am. J. Sci. **241**: 413–429. 1943.

plex problem than that of South American-North American relationships. There is a very large literature on this subject, but the best general reviews of it are now far out of date,[2] and much of the literature is fragmentary, excessively subjective, or otherwise inadequate. There are two main lines of evidence, which are frequently confused: first, earliest dates of common appearance of given groups on both continents, which are limiting dates for the migrations of these groups; and, second, fluctuations in faunal resemblance of the continents. In each case, not only the overall statistical picture, involving numbers of migrant groups and degrees of resemblance, but also the nature of the animals concerned, especially their ecological and climatic preferences, are pertinent.

An attempt has been made to place this study on a more objective and quantitative basis than heretofore. There are various difficulties and shortcomings in the quantitative approach. These have to be studied carefully, and applied with due caution, but preliminary results are encouraging. The work is still under way, and it would be premature, now, to give any detailed conclusions. Some of the preliminary results, however, seem likely to stand up.

As regards frequency of migration between Eurasia and North America, preliminary study indicates an early Eocene climax, a mid-Eocene low, a dual late Eocene-early Oligocene climax, a middle-to-late Oligocene low, slow resumption in the early Miocene, building steadily to a dual late Miocene-early Pliocene climax, another dual late Pliocene-Pleistocene climax, and then a drop to the Recent low. Migration is only one of at least five separable factors involved in faunal resemblance. Faunal resemblance does not vary directly with the frequency of intermigration, and its measurement and analysis are particularly complex and difficult, although reasonably good results have been obtained and better are in sight.

The presence, or absence, of a land bridge and, when present, its position, do not follow as directly from the evidence as to frequency of migration and degree of faunal resemblance as has commonly been supposed. As far as this new analysis of the faunal data has yet been followed, it suggests that there was a Eurasian-North American land bridge almost continuously throughout the Cenozoic, with probable

[2] Notably: **Osborn, H. F.**, *The Age of Mammals*. Macmillan. New York. 1910.

interruptions of appreciable duration only in the middle Eocene, middle to upper Oligocene, and Recent. As regards the position of the bridge, or bridges, it is a surprising result of this study that no good evidence is found for any Atlantic, direct European-North American, bridge during the Cenozoic, contrary to my own former opinion and that of most paleogeographers. The evidence throughout is consistent with a single Siberian-Alaskan bridge, as adequate to explain all the known facts.

SECTION OF BIOLOGY

MAY 13, 1946

Symposium on *"The Place of the Standard Dog in Medical Research."* The program consisted of the following papers*:

"The Physiological and Biochemical Standardization of the Dog," by James B. Allison, Bureau of Biochemical Research, Rutgers University, New Brunswick, N. J.

"Phases of Conditioned Reflexes of Dogs used in Medical Research," by Hans Molitor, Merck Institute for Therapeutic Research, Rahway, N. J.

"Some Practical Aspects of Selecting and Maintaining Dogs for Medical Research," by Mark L. Morris, New Brunswick, N. J.

* The papers by Doctor Allison and Doctor Morris are published in this number of the Transactions. No abstract of the paper by Doctor Molitor has been received.

Doctor James B. Allison, Bureau of Biological Research, Rutgers University, New Brunswick, N. J.: *The Physiological and Biochemical Standardization of the Dog.**

A scientist never uses a delicate apparatus in the laboratory without carefully checking and calibrating it. Too often, however, an animal, such as the dog, is used as an instrument of measurement without much knowledge of its physiological state. There is no more delicate, nor complex machine than the living system, a machine that requires much thoughtfulness and knowledge to use properly in the laboratory. This machine, that we call living, can be described, grossly, as a dynamic equilibrium between cells and the media in which they function. Thus, body fluids reflect the well-being of the community of cells which is the animal, abnormal behavior of the cells being detected by changes from normal in the composition of the body fluids. A study, therefore, of these fluids contributes much to an understanding of the physiological state of the experimental animal. It is toward this understanding that the following review of the composition of the blood and urine of dogs in different physiological states is directed. It is a review of elementary attempts to calibrate the dog for experimental purposes in medical science.

The variations in the volume and composition of the body fluids are relatively large in a heterogeneous group of so-called normal dogs. A normal range of variation is often determined for such groups. However, it is the purpose of this paper to point out that changes within that range may be important in an experiment; that, although large numbers of dogs are often used to get statistically significant results, a few carefully calibrated animals can tell the story more truthfully and accurately. To do this, each dog should be placed in as constant a physiological state as possible, and the range of variation established for that state. Similar physiological states cannot be achieved, however, by putting every animal under the same experimental conditions. The caloric intake of the diet, for example, necessary to maintain a population of dogs in an optimum state of nutrition, can vary, individually, from 40 to 150 calories per day per kilogram of body weight. One dog may require much more protein than another to maintain nitro-

* These studies were supported in part by the Protein Metabolism Fund of the Bureau of Biological Research.

gen equilibrium. Thus, the caloric intake and nitrogen intake must be adjusted for each dog, to bring the population into similar physiological states. Other dietary factors must also be controlled. Hence, a semi-synthetic diet[1] is recommended for carefully controlled experiments.

The plasma volume was found by Gregersen and Stewart[5] to vary between 35 and 65 ml. per kg. of body weight in a heterogeneous group of dogs. However, as these authors point out, individual variation is not nearly so large. Indeed, under controlled conditions, plasma volumes can be kept constant in single individuals within the error of the determination. The data in TABLE 1 illustrate the agreement which may be expected in plasma volumes and "available fluid volumes," in dogs kept under such conditions. One of the changes which will effect plasma volume markedly is a shift in protein stores of the animals. Reduction in plasma albumin, for example, is reflected by a fall in plasma volume, and a rise in available fluid. A complete description of these shifts in fluid balance, which accompany changes in protein stores, will be published elsewhere. It is, however, important to note here that changes in labile protein stores in the dog during an experiment can alter markedly the volume of body fluids.

TABLE 1

PLASMA AND AVAILABLE FLUID VOLUMES IN FOUR DOGS

(Sample 2 was taken 5 days after sample 1. Plasma volumes were determined by using the dye T1824; available fluids, by using Na SCN.)

Dog No.	Sample No.	Plasma volume (P)	Available fluid (A)	A/P
28	1	529	2470	4.7
	2	527	3090	5.9
42	1	500	2700	5.4
	2	509	2640	5.2
65	1	557	2700	4.9
	2	560	3000	5.4
68	1	749	3140	4.2
	2	731	3850	5.3

The volume of red blood cells in a population of laboratory dogs varies considerably, averaging around 45 per cent of the whole blood. Individual volumes, also, can vary from time to time, because of the

pooling of these cells in the spleen. However, under controlled conditions, they are quite constant. The reduction in hemoglobin and red blood cell count, as well as leucocyte count, during barbiturate anesthesia, illustrated in TABLE 2, is an exaggerated example of the pooling of these cells in the dilated spleen. The data in the literature[6] demonstrate that the red blood cells increase in numbers, from a few million per c.mm. of blood in young puppies, to an average of 6.2 million in the adult dog. The average corpuscular hemoglobin is 24×10^{-12} gm. in both young and old dogs. The hemoglobin concentration of the blood, therefore, increases from 9 to 10 gm. per 100 ml. of blood, at 2 months of age, to an average of 15.1 gms. in the adult animal. These are all so-

TABLE 2

AVERAGE DATA OBTAINED ON THREE DOGS GIVEN 48.8 GMS. OF DELVINAL SODIUM PER KGM. OF BODY WEIGHT

(*Data taken from* Allison, Seeley, and Morris,[4] *1944*)

Condition of dogs	Hemoglobin	Red blood cells	White blood cells
	gms./100 ml.	Millions per c. mm.	Thousands per c. mm.
Normal	14.6	6.2	15.9
Deep sleep	12.3	5.3	9.7
Awakening	12.6	5.1	10.7
Recovered	14.3	6.3	16.0

called normal values. It may be, however, that the low red blood cell counts so often recorded for young dogs are due in part to dietary deficiencies. Recently, Nakamura, Morris, and Atkinson,[8] for example, reported red cell counts as low as 1.5 to 2.5 million per cubic millimeter, and hemoglobin as low as 7.5 gm. per 100 ml., in puppies receiving milk from a mother fed an inadequate diet. A quick and complete regeneration of cells and hemoglobin was accomplished by treating the puppies twice a week with liver extracts. Regeneration of this type, however, can take place only in the presence of an adequate intake of the right kind of protein. It has been our experience that regeneration is most rapid if the animals receive, daily, at least 0.4 grams of protein nitrogen per kgm. of body weight, together with a salt mixture containing iron, copper, and manganese, and with adequate vitamin therapy.

The white blood cell count, both total and differential, is one of the most useful tools to determine the condition of the dog. Using the

Schilling method of classification,[6] the polymorphonuclear neutrophils are classified as myocyte, juvenile, stab, and segmented forms. The first two forms, however, are rarely found in the blood of the normal dog. The average white count for the normal adult dog is 11,467 (TABLE 3), just about twice that found in man. There is a tendency for young dogs to have a slightly higher white count, with a greater

TABLE 3

COMPARISON OF THE BLOOD PICTURE OF NORMAL DOGS AND DOGS SUFFERING FROM DISTEMPER

(Data compiled primarily from records kept at the Raritan Hospital for Animals, Stelton, N. J.)*

	2–8 months				9 months and older			
	Normal 35 dogs		Distemper 80 dogs		Normal 31 dogs		Distemper 80 dogs	
	Average Number	Average per cent	Average Number	Average per cent	Average Number	Average per cent	Average Number	Average per cent
W.B.C.	12165	—	12611		11467	—	12123	
Seg.	6795	55.85	8115	64.35	7525	65.27	8451	69.71
Lymph.	4051	33 30	2050	16.26	2491	21.72	1492	12 31
Stab.	816	6.70	2050	16.26	753	6.56	1652	13 63
Eosin.	484	3.97	71	0.56	623	5.43	180	1 48
Mono.	19	0.15	147	1.17	77	0.67	145	1.20

proportion of lymphocytes, and fewer segmented forms than adult dogs, the numbers of lymphocytes being more labile, as they respond more to changing physiological conditions in young, than in adult, dogs.

A decrease from normal in the percentage of lymphocytes, and the increase in percentage of stab cells, are characteristic of dogs suffering from distemper. The average total number of white blood cells recorded in TABLE 3 are not changed from normal in the distemper dogs. Nevertheless, these average figures can be misleading, since, with a pure virus distemper, the total number of white blood cells is always reduced below the normal range. In virus distemper, the number of white blood cells is reduced, accompanied by a reduction in the percentage of lymphocytes and an increase in neutrophils. A dog in this physiological state is very susceptible to secondary infections. These infec-

* Acknowledgement is made to Dr. M. L. Morris of the Raritan Hospital for Animals, for permission to compile these data.

tions usually cause an increase in the total numbers of white blood cells, often increasing them well above normal. Thus, a leucocytosis may accompany distemper, if secondary infections are present, but, in a heterogenous population of distemper dogs, white blood cell counts may range from below, through average, to well above normal. The differential, however, regardless of the white count, will always show the characteristic "shift to the left."

A marked leucopenia and a polymorphonuclear leucocytosis is an unfavorable response to distemper. Dogs in this condition can appear normal to the laboratory worker, but are very susceptible to secondary infections, developing meningo-encephalitis,[7] and other diseases. It is not uncommon for a dog suddenly to sicken and die, after apparently recovering from distemper. Such animals are always in a condition which produces a marked lymphocytic leucopenia. Indeed, a low lymphocyte count in a dog is always associated with lowered resistance to disease, possibly reflecting lowered antibody formation. Certainly, such dogs, when depleted in proteins, are very susceptible to infections. One dog, apparently normal, but with a low lymphocyte count, placed on a protein-free diet, always developed a severe sore throat, which could be cured only by repletion on a good protein diet, such as whole egg.

Depletion in body protein stores is reflected by a reduction in plasma proteins including antibody proteins, reductions in plasma volume, increase in available fluid volume, reduction in excretion of body nitrogen, and a decrease in resistance to disease.[2] Wounds heal very slowly in depleted dogs, sores sometimes called kennel sores developing in their skin. It is not unusual to find pound and even laboratory dogs in this depleted condition. If such dogs are used as experimental animals, they will yield results which are quite different from those obtained on animals with adequate protein stores. The nitrogen balance index, for example, of casein determined in a normal dog is 0.8, while in a protein-depleted animal this index may be as high as 1.0.[3] In general, if the plasma proteins are less than 5.5 grams per 100 ml., the nitrogen balance index will be abnormally high and the excretion of urea nitrogen abnormally low.

Many other examples of biochemical and physiological standardizations of dogs, correlated with physiological states and experimental results, could be given, but would be beyond the bounds of this short

paper. It might be helpful, however, to reemphasize the necessity of a careful study of the experimental variables under controlled conditions. The data in TABLE 4 were obtained on four dogs, during a period of approximately two weeks, while they were being prepared for an experiment which involved, among other variables, the hematocrit, blood urea, and liver function tests. The data in the table show that the per cent red blood cells are normal and quite constant; yet, there may be a tendency, ever so slight, for the per cent cells to decrease over this two

TABLE 4

HEMATOCRIT, BLOOD UREA, AND BROMSULPHALEIN TESTS ON FOUR DOGS DURING A CONTROL PERIOD OF APPROXIMATELY TWO WEEKS

Dog No.	Red Blood cells, %			Blood urea, mgs. %			Bromsulphalein test, %		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1	46.9	45 4	45 1	16 9	17.6	17.3	2	9	7
2	44.9	45 0	42 3	10 6	10.4	11.2	19	23	15
3	45 9	46 0	44 4	9 3	8 2	8 8	4	10	9
4	49.5	47.5	46 3	8.6	7 4	9 0	8	—	12

week control period. This slight decrease warrants further investigations, especially since it is possible that the vitamin content of the diet and the intestinal flora of the dog may have been deficient in certain hemoglobin-forming factors. The blood urea nitrogen concentrations are all within the normal range for the four dogs, and yet dogs 1 and 2 have higher blood urea nitrogens than dogs 3 and 4. Preliminary studies, which will be amplified elsewhere, indicate that these differences are correlated with the utilization of dietary nitrogen in the presence of varying protein stores of the animal. Finally, dog 2 is separated from the other three by the bromsulphalein test for liver function, this dog having a decreased liver activity toward the dye.

Thus, though variables may be within a so-called normal range, significant differences can be established within that range. Each animal must be considered as a complete instrument of measurement, which records significant results. The interpretation of these is dependent upon knowledge of the state of the instrument at the time the record is made. A few carefully calibrated animals will tell the story more truthfully and accurately than a large heterogeneous group, no matter how clever the statistical treatment of the data from the group may be.

LITERATURE CITED

1. **Allison, J. B., & J. A. Anderson**
1945. J. Nutrition **29**: 413.
2. **Allison, J. B., J. A. Anderson, & R. D. Seeley**
1946. Ann. N. Y. Acad. Sci. **47**: 3. (In press.)
3. **Allison, J. B., R. D. Seeley, J. H. Brown, & J. A. Anderson**
1946. J. Nutrition **31**: 237.
4. **Allison, J. B., R. D. Seeley, & M. L. Morris**
1944. Am. J. Vet. Res. **5**: 62.
5. **Gregersen, M. I., & J. D. Stewart**
1939. Am. J. Physiol. **125**: 142.
6. **Morris, M. L., J. B. Allison, & D. F. Green**
1940. J. Lab. & Clin. Med. **25**: 353.
7. **Morris, M. L., & T. J. Murray**
1940. J. Am. Vet. Med. Assoc. **96**: 80.
8. **Nakamura, F. I., M. L. Morris, & L. N. Atkinson**
1945. J. Am. Vet Med. Assoc. **106**: 336.

DOCTOR MARK L. MORRIS, New Brunswick, N. J.: *Some Practical Aspects of Selecting and Maintaining Dogs for Medical Research.*

The practical aspects of selecting and maintaining dogs for medical research will be considered under five headings.

The *availability* of suitable dogs constitutes a problem, and the situation is becoming increasingly more difficult. In the past, dogs have been available from three sources, the principal one being the animal dealer. Such a person makes a business of buying, raising, and shipping dogs to institutions or laboratories. The supply from these sources is not too adequate, and the quality of animals obtained is, for the most part, unsatisfactory. The initial cost per dog is reasonable, in fact, very cheap. However, the mortality rate of the purchased stock is high, often reaching 75–100%. If *all* of the expense is assigned against the conditioning of the dog, up until the time the animal is successfully employed for study, then the total cost is much greater.

Dogs are also obtained from pounds and shelters. Such supplies are even more unreliable: numbers available fluctuate widely, and the quality is usually unsatisfactory. The mortality rate of animals from pounds is perhaps even higher than of those obtained from animal dealers, because most dogs that find their way into pounds are usually susceptible to infections commonly inhabiting the premises. The initial cost is cheap; but, again, the end cost per animal successfully used is quite high, perhaps even higher, than of those obtained from animal dealers. When dogs are purchased from wardens in charge of pounds, such a practice encourages the collecting of dogs from the street. This may be a desirable practice; but the average pet animal owner permits his dog to use the street as an exercising yard, and, consequently, these pet animals are frequently impounded and subsequently reach a research laboratory. If the requirement of laboratories is active and the supply is short, in an effort to meet the demand it is not uncommon for the unscrupulous warden or his employee to remove and destroy collars bearing identifying licenses. It is unnecessary to point out that such a practice creates animosity in the minds of the public, and they turn for relief to the various societies organized for the purpose of protecting the welfare of animals. These organizations have influen-

tial public relations contacts, so that appeals can be made personally, or through the press or the radio. In an effort to assist the complaining public, the societies seize upon the opportunity to be of service, making a target of research laboratories and their personnel. A serious public relations problem is created, and the laboratory is forced into a defensive position. If the animals originated as the private property of the laboratory, there would be less criticism from animal welfare societies.

The supply of dogs from privately owned kennels is, at present, very limited. Owners and breeders are usually not interested in raising dogs for medical research, but breed the animals for show purposes, or to be sold as pets. In fact, many would seriously object to selling dogs to laboratories, if the animals were to be used for medical research. The present and future supply of dogs from privately owned kennels may, for the most part, be largely discounted. It can, thus, be stated that the majority of animals at present employed in laboratories are obtained either from animal dealers, or from pounds and shelters.

The *housing* of animals used by laboratories may be considered under three headings: general physical facilities, individual kennels, and exercising yards. The general physical facilities, from what I have observed, vary widely, the most common being cellars, attics, and back rooms. These animal houses often have no light, poor ventilation, and unsanitary conditions. In contrast, there are some laboratories which have modern facilities, providing light and ventilation, and the construction is such that floors and walls can be washed and kept sanitary. The individual kennels vary from either single cages or metabolism units to wire enclosures for confining groups of from five to twenty dogs. The large enclosure may be practical, but it encourages fighting, breeding, dissemination of disease, and poor feeding habits. Under these conditions, the outcome is frequently a survival of the fittest, the larger and stronger dog gulping the food, the smaller and less fortunate animal surviving on left-overs.

In most instances, yards or facilities for exercising are not provided. If present, these units are usually small indoor enclosures, either with or without drainage, constructed to accommodate 20 or more animals, and thus encouraging a high concentration of excreta in a small area. Outdoor exercising yards with fresh air, natural light, and good drainage, are essentially non-existent. Visitors to research laboratories cannot help but feel sympathetic toward creatures con-

fined under crowded unsanitary conditions. In an effort to improve the care and handling of the dogs, an employee or staff member is often instrumental in offering suggestions to qualified persons. This encourages an investigation, and develops adverse publicity for the laboratory, the personnel, and for the practice of using dogs for medical research.

The *feeding* of dogs used by laboratories is very important. This problem may be considered under two headings: the quality of the food, and the method of feeding, both prior to purchase and during the animal's stay in the colony. The institution purchasing the dog has no control over the quality of food or method of feeding. However, each has an important bearing on the condition of the dog, at the time of purchase, and for some time subsequent thereto. When the animal is added to the colony, its nutrition varies widely, depending upon the food available and the purpose for which the dog is to be used. Usually, the ration supplied by the institution is of unknown biological quality, having been purchased from a producer who is a good customer, client, or friend of the company, or because the particular ration is one employed by some other well-known laboratory. The palatability and nutritional quality are too frequently secondary, so long as the food is reasonable in price and the animals consume the ration and continue to live.

Disease control is obviously fundamental, and must be given consideration, both prior to purchase and during membership of the colony. Control of disease in dogs requires time, skill, and expensive materials. Animal dealers, dog wardens, or pound keepers are not interested, and the housing and feeding techniques employed by such persons encourage the development of disease. It is difficult to obtain a dog from a pound that has not been exposed to, or is incubating, a parasitic, bacterial, or virus-induced disease. The desire of the animal dealer is to turn the dogs over as rapidly as possible, and collect the fee before the animal becomes ill and dies. Towards an addition to the colony, some attempt at control is made by most institutions. However, the additions continue to supply ample quantities of viruses, bacteria, and parasites to nutritionally deficient stock. Old resident members of the colony are usually immune to "everything in the book."

Experimental animals are fed, prior to purchase, all sorts of rations of unknown values, while those maintained in laboratory colonies

are fed rather empirically. Disease control is on a somewhat comparable basis, little effort being made by the animal dealer. Most institutions have inadequate isolation and housing facilities, plus an insufficient number of qualified trained personnel to cope with the problem. As a result of malnutrition and disease, many dogs used for research purposes suffer from varying degrees of organic damage, chiefly of the kidney and liver. These functional changes directly affect the biological status of the animal, so that the results of many physiological, pathological, and pharmacological investigations are adversely influenced. Thus, it can be stated that the average dog used for medical research is a malnourished, diseased, pathological specimen, often improperly housed and nourished, but used to obtain physiological and pharmacological data. In selecting dogs for various projects, the physiological state is not taken too seriously, the investigator's criterion being frequently: "That one looks okay; we'll use him." Subsequently, however, the animal proves unsatisfactory, fails on the project, and is discarded. The cost of the dog, preliminary preparation, and the investigators' time and materials are a total loss. If the investigation is completed, one is never certain of the results; attempts to repeat the study on the same animal or one of the same species fail to produce comparable data. It is surprising that more employees are not bitten by rabid animals, since it is the function of the dog warden to remove stray animals from the streets. These campaigns are vigorously carried on when rabies is most prevalent. Leptospirosis and certain fungus infections may also be contracted from diseased dogs, and these conditions are frequent among pound animals. Certainly, some hazard to employees must exist.

A critical analysis of the problem is essential, if a solution is expected. Is there any approach, or can any recommendations be made, which would assist in improving the biological status of dogs now widely employed in medical research? If laboratories wish to continue the present practice of purchasing animals from dealers, pounds, or shelters, then some system for screening should be devised which will immediately eliminate the most evident pathological specimens. It will be almost impossible to obtain diet histories on animals procured from dealers or pounds. Therefore, some plan should be devised for rehabilitating the malnourished and diseased. These procedures should be undertaken in quarters entirely separate from the

main colony. Disease should, in so far as possible, be eradicated, and proper immunization applied.

In addition to improving present methods, it would seem that some thought might well be given to the development of a so-called standard-bred animal of known genetic background, free of parasitic or infectious diseases, properly immunized, and fed a balanced, nutritionally complete diet. Such an animal should be of convenient size to be tractable. It should have good veins and arteries, understanding, and a tolerant disposition. Temperamental, neurotic creatures should be avoided. A good, rugged, disease-resistant strain, possibly developed by cross-breeding certain pure strains, might prove ideal. It is well known that the dog is the most valuable animal employed in medical research for obtaining reliable data applicable to the human. To accomplish these purposes, a properly organized, concentrated effort must be made.

On several occasions, representatives of institutions and industries have discussed this problem with us. It would seem that a closely-knit program, sponsored by industrial and institutional laboratories, is needed to develop, finance, and administer a program which would make dogs of uniform size, scientifically fed, and free of disease, available for medical research. Millions of dollars are expended annually on various types of instruments and mechanical equipment. Is it not logical that a comparable effort should be made to improve the quality, feeding, housing, and methods of handling animals which make a major contribution to medical research and human welfare?

Appreciation is expressed to my colleagues and friends who offered comments and criticisms.

SECTION OF PSYCHOLOGY

May 20, 1946

Doctor Edmund Jacobson, Laboratory for Clinical Physiology, Chicago, Illinois: *"Electrical Measurements of Mental Activities in Man.* (This lecture was illustrated by lantern slides.)

The view that mental activities, such as imagination, perception, recollection, and emotion, occur in and through the brain alone has been almost universal for centuries. During a series of observations of sensory experiences which I began in 1908, and in which I later had the assistance of graduate students and faculty members whom I trained especially for the purpose, I was led to doubt the accuracy of the traditional view. I believed that I could detect sensory and motor experiences, evident, not in the brain, but in skeletal and smooth muscles and elsewhere, which seemed indistinguishable from the conscious processes present during the mental activity under observation, and which seemed, to all intents and purposes, to constitute all or part of the mental activity itself. However, these subjective observations, no matter what the controls, failed in themselves to constitute proof that mental activity is not confined as a function to closed circuits within the brain. Objective tests, based upon the subjective findings, were obviously the next step.

Accordingly, after many years of effort, electrical apparatus of unique sensitivity was devised and set up in 1927, capable of measuring transient voltages as small as one millionth of a volt, under special conditions and limitations. Suitable electrodes were devised, and placed in contact with skin overlying muscles to be tested. In later studies, wire electrodes were inserted into the muscles or into the superficial nerves which were under investigation.

During the mental activity studied, which generally lasted about a second or more, action-potentials were discovered in the muscle or muscles which would naturally participate if the actual act were to be carried out, but in an abbreviated and fainter fashion. For example, if the subject imagined that he was lifting a ten-pound weight with his right hand, action-potentials would arise promptly after the recorded

signal to imagine and would cease promptly after the recorded signal to discontinue to imagine. However, this did not occur uniformly in all subjects, for some reported that they merely visualized themselves lifting the weight. In these, the electrical records generally indicated merely eye-movement to imagine. Other subjects showed electrical phenomena, as described, simultaneously in the eye and arm regions.

With the aid of sensitive mechanical contrivances, it was shown that the action-potentials recorded in electrodes connected with a muscle were accompanied by actual, but minute, contractions in that muscle. In the instance where the subject was instructed to imagine lifting a ten-pound weight with his right hand, the muscles on the right side actually contracted minutely, as if to lift that weight. But in relaxed subjects, the muscles on the side showed no electrical or mechanical signs of contraction.

Mental activities referring to abstract matters were commonly carried out in terms of eye or speech organ activities which could be electrically recorded and identified.

Since these earlier investigations, many subjects have been trained to relax, as is essential for the many types of control tests necessary in these investigations. When the subject relaxes his skeletal musculature to a very advanced degree during a certain interval, he later reports that mental activity was diminished or absent for the time. Corresponding with the subject's report, action-potentials are generally diminished or absent in the specific region under test.

Evidence has been found in this laboratory that individuals, both healthy and diseased, can be trained to relax. Individual differences obtain, but a general trend is manifested in electrical recordings in most instances. Training effects an increased ability in the individual to control his mental activities in the direction of relaxation. This can have practical applications.

SECTION OF ANTHROPOLOGY

May 27, 1946

Doctor Ruth Benedict, Associate Professor of Anthropology, Columbia University, New York, N. Y.: *The Study of Cultural Patterns in European Nations.*

Every nation in Europe and Asia has simultaneously denied and boasted that it had a national character. It has been almost impossible to separate the wheat from the chaff in the extravagant statements that have been put forth, and many social scientists have been inclined to chalk up the whole problem as a subject for popular oratory, and to throw it out of court as a matter for systematic investigation. During the war years, however, the problem of national character became a matter of grave practical importance. There were crucial questions as to "the nature of the enemy," the receptivity of satellite nations to certain kinds of appeals and not to others, and the opposition of certain of the allied nations to measures easily accepted by others. Those who were engaged in psychological warfare, in political conferences, in the training of OSS and UNRRA personnel, as well as those engaged in military operations, were constantly handicapped if they made mistakes in estimating the way a *vis-à-vis* nation would think and behave. It was of the utmost importance to eliminate popular fantasies and misapprehensions, and to use whatever techniques the social sciences could offer, in order to understand these national characters.

To the anthropologist, the study of national character is a study of learned cultural behavior. For several decades before the war, anthropologists had done pioneer work, in this field, in compact primitive communities. During the last decade, theoretical points made by anthropologists about cultural conditioning had been widely accepted Anthropologists had presented their case convincingly enough so that there was wide agreement that social arrangements are of fundamental importance in shaping any people's tenets about life, whether they are assumptions about the function of the State, economic motivations, relations between the sexes, or dependence upon the supernatural. The

forms these tenets take in our own cultural background were no longer generally considered to be direct consequences of human biology, and "human nature" was no longer considered as a sufficient explanation of them. Behavior, even in civilized nations, was increasingly understood as ways of acting and thinking which developed in the special kind of social environment characteristic of that part of the world.

In 1943, I was asked to join the Office of War Information, to work on national character in enemy and occupied countries. I was asked to use the insights and techniques anthropology had developed in the study of learned cultural behavior. In spite of all the necessary limitations imposed upon research in the social sciences during war, it was a great opportunity. Studies of modern society had very seldom, indeed, made systematic use of the methods upon which anthropologists had based their analyses of the simpler societies. These methods were quite specific, and were designed to investigate how each new generation had learned and transmitted its way of life in all its specificities. They were methods for detailed studies of specific social environments. Experience had shown that it was necessary to stress many aspects of life which rate as trivia in Western international studies. Habit formation in a specific social environment; the rewards and punishments bestowed by society; the praise allotted to certain kinds of achievement; the connotations given to exercise of authority, and to submission to it, in day-by-day living; the degree to which responsibility for his own conduct was entrusted to the individual—all such questions had been regarded as essential in cultural investigations of behavior in primitive societies, and had hardly been raised in studies of European nations. In classic studies of civilized countries, the approach is, ordinarily, either historical, or economic, or political. Though such segmented approaches are valuable and necessary, they still leave a wide field for applications of methods which have been successfully used in anthropological studies of learned cultural behavior.

In attempting, during the war, to use such techniques in the study of civilized nations, there was a grave handicap, occasioned not by the nature of the research, but by the fact of war. The anthropologist's chief technique, that of the field trip, was impossible. There were available, however, in the United States, persons of almost every nation of the world, and it was a fairly simple matter to find transplanted groups which retained a great deal of the way of life to which

the older members had been born. Individuals could be found from most classes and minorities, and from most of the distinctive provinces of a nation. It was not necessary to give up the traditional anthropological reliance upon face-to-face study, and this recourse to informants was all the more necessary, the clearer it became that much essential material for the studies I had been asked to make was not elsewhere available.

The usual comment on such projected studies of civilized countries is that, quite apart from the limitations imposed by the war, civilized nations are too difficult to study by methods that may be sufficient in small communities. Such skepticism is often based on what should rather be regarded as a great advantage: the multiplicity of the facts known and recorded about Western nations. Actually, the anthropologist working on civilized nations has a great head-start, in that much work has been done in historical research; that statistics are available in many fields; that so many observers have recorded their personal experiences; that there are often excellent novels available; and that the language does not present the grave obstacles it does in tribes where it has never been recorded and ordered in grammatical categories. Vast quantities of material are a handicap only when the crucial problems to be investigated are not formulated. When they are, it is possible to cull the relevant material from the most diverse sources. The richness of the data is an asset, and, when lacunae were discovered, it was usually possible to obtain necessary facts from informants. The principal advantage the anthropologist had, was that certain ways of stating the problems had emerged from his experience, and stating the problem so that it can be answered by research is usually half the battle.

Skepticism about the application of anthropological techniques to civilized nations is also often based on the lack of cultural homogeneity in modern nations. This kind of skepticism frequently seems to the anthropologist to be no criticism of his method, but a statement of an elementary principle which he completely accepts. No anthropologist, I think, would attempt to study "the" character structure of such a welter of cultures as were included within the national boundaries of Jugoslavia. There are other multicultural states such as Poland and Czechoslovakia. Such conditions do not mean that investigation must be abandoned. The solution is to multiply the number of investiga-

tions to any desired point, and this holds true also of such lesser problems as are presented by the different regions of England, or of France, or of the United States.

The criticism that the degree of class differentiation prevalent in Western civilization makes the use of anthropological methods impossible, stands on a somewhat different footing. Adequate cultural study of this situation, including all the relevant factors, has hardly been attempted in Western nations. Such a study would investigate what attitudes and convictions the various classes have in common in any nation, as well as the obvious fact of conflict of interests. Even the conflict situation is usually inadequately stated. The trained anthropologist, in any study of complementary behavior, whether between authoritarian fathers and submissive sons, or between despotic kings and their subjects, has to present both parties as actors in a patterned situation. He can see it as a kind of see-saw, and by studying the height of the fulcrum and the length of the board (in the study of classes, laws about property and land, general conditions of social security, and the like), he can show either that the group on the high end of the see-saw is necessarily very far up and the group on the low end very far down, or that they are more nearly balanced. As extremes, material or psychological, are eliminated from one position, extremes will also be eliminated from the other. Other groups in the society, too, may throw their weight now to one party and now to the other. The anthropologist has good reason to know, also, that nonmaterial factors may be as important as material ones in any given situation, and he investigates, for instance, the cultural acceptance of hierarchy, as well as the relative frequency of wealth and poverty.

The similarity of the basic assumptions about life made by both those classes, in any nation, is of great importance. The wealthy industrialist and the laborer or peasant, in a nation or area of Western civilization, hold many attitudes in common. The attitude toward property only in part depends upon whether one is rich or whether one is poor. Property may be, as in Holland, something which is an almost inseparable part of one's own self-esteem, something to be added to, kept immaculately, and never spent carelessly. This is true, whether the individual belongs to court circles or can only say in the words of a proverbial expression: "If it's only a penny a year, lay it by." Alternatively, the attitude toward property may be quite dif-

ferent, as in Roumania. An upper-class person may be, or become, a pensioner of a wealthy man, without loss of status or self-confidence; his property, he says, is not "himself." And the poor peasant argues that, being poor, it is futile for him to lay anything by; "he would," he says, "if he were rich." The well-to-do increase their possessions by other means than thrift, and the traditional attitude toward property differences associates wealth with luck or exploitation, rather than with assured position as in Holland. In each of these countries, as in other European nations, many of which have deeply embedded special attitudes toward property, the specific nature of these assumptions can be greatly clarified by study of what is required of the child in his handling and ownership of property, and under what sanctions and conditions expanding opportunities are allowed in adolescence, and at his induction into fully adult status.

Attitudes toward authority are similarly localized. A Greek, whether he belongs to the upper classes, or whether he is a peasant villager, has a characteristic opposition to authority from above, which permeates daily conversation and influences his choice of a means of livelihood quite as much as it colors his political attitudes. On the other hand, it is quite true that, in other regions of Europe, in the dramatic words of Ortega y Gasset, there has been a "formidable cry rising like the howling of innumerable dogs asking for someone or something to take command, to impose an occupation, a duty." During the war, Goebbels's propaganda broadcasts quoted the well-known words of Machiavelli, saying that all Germans knew they were true: "Men work either under compulsion, or of their own will. The greatest energy they display where their own choice has the least freedom." Such authoritarianism deserves the closest cultural study. It requires knowledge, not only of the laws and of the economic set-up that have fostered it, but of the child's first experiences with authority, and of the sanctions which are invoked. It requires knowledge of the age at which various disciplines are imposed, and of the rewards of obedience. Such knowledge can lead directly to a clearer insight into what the leaders in any country are saying in their political speeches, and into what courses of action the people of that country can advantageously follow in reconstruction. Character structure can, of course, change over generations, as different experiences are provided, but the very process of change can be illuminated by systematic study of behavior in this generation.

At present, even the recorded facts necessary for a cultural study of the nations of Europe are widely scattered in different publications, and much crucial information is not recorded at all. Our understanding of international affairs is about where our understanding of primitive peoples was before the anthropologists attempted the serious study of how primitive people *learned* their cultural behavior. Even those students who have used the method in the simpler cultures have usually laid it aside when they came to discussions of our own civilization. They tend to assume a similarity in experience among the different Western nations, which my investigations showed did not exist. There are differences of the same order as those with which we are familiar in the isolated simpler cultures. We need studies of Western peoples which show them to us as people who have learned, in specific ways, to solve the universal human problems by special cultural arrangements to which they give their allegiance as we do to ours. We need intimate understanding of their experiences, so that we shall learn to discriminate between what is truly socially dangerous and what is only another method of arriving at a socially desirable goal. The kinds of strength which the people of each area could use in a world organized for peace can only be those to which they have been bred. If we insist that they imitate another kind of strength, they will be powerless to contribute. If we, the people of the world, are ever to achieve a world organization which promises mutual benefits, we must be scientifically prepared to know the strength which different nations of the world can utilize to this end.

SECTION OF OCEANOGRAPHY AND METEOROLOGY

May 24 and 25, 1946

Conference on *"Convection Patterns in the Atmosphere and Ocean."*

The Section of Oceanography and Meteorology held a Conference on "Convection Patterns in the Atmosphere and Ocean." Doctor Athelstan F. Spilhaus, New York University, College of Engineering, New York, N. Y., was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

Friday, May 24

Morning Session. Chairman, Athelstan F. Spilhaus.

"Conference Introduction, Problems Concerning Convective Layers," by R. B. Montgomery, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"A Summary of the Theory of Convection Cells," by Henry Stommel, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Internal Waves in the Atmosphere and Convection Patterns," by B. Haurwitz, Massachusetts Institute of Technology, Cambridge, Massachusetts.

Afternoon Session. Chairman, J. Bjerknes, University of California, Los Angeles, California.

"Convection of Aerosols," by Worth H. Rodebush, University of Illinois, Urbana, Illinois.

"Convective Motion in Air over the Sea," by A. H. Woodcock and Jeffries Wyman, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

"Structure of Summer Rain as Detected by Radar," by Raymond Wexler, Evans Signal Laboratory, Belmar, New Jersey.

"Observations of Vertical Temperature and Humidity Distributions in the Convective Layer above the Sea Surface," by Richard A.

Craig, Woods Hole Oceanographic Institution, Woods Hole, Massachusetts.

SATURDAY, MAY 25

Chairman, H. U. Sverdrup, University of California, The Scripps Institution of Oceanography, La Jolla, California.

"Convection in the Annual Temperature Cycle of Lake Michigan," by Phil E. Church, University of Washington, Seattle, Washington.

"The Maintenance of Instability in the Surface Waters of the Ocean," by John C. Armstrong, American Museum of Natural History, New York, N. Y.

SECTIONS OF BIOLOGY AND PHYSICS AND CHEMISTRY

May 29, 1946

Conference on *"Folic Acid."*

The Sections of Biology, and Physics and Chemistry held a Conference on "Folic Acid." Doctor E. H. Northey, American Cyanamid Company, Stamford, Connecticut, was the Conference Chairman, in charge of the meeting.

The program consisted of the following papers:

"Historical Introduction," by W. H. Peterson, University of Wisconsin, Madison, Wisconsin.

"Isolation of Liver *L. Casei* Factor," by E. L. R. Stokstad and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Isolation of Fermentation *L. Casei* Factor," by Brian Hutchings and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Degradation of *L. Casei* Factor by Alkaline Hydrolysis," by E. L. R. Stokstad and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Degradation of *L. Casei* Factor by Sulfite Cleavage," by Brian Hutchings and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Structure of *L. Casei* Factor," by J. H. Mowat and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Synthesis of Pteroic Acid and its Derivatives":

First paper, by Coy Waller and collaborators, Lederle Laboratories, Pearl River, N. Y.

Second paper, by M. E. Hultquist and collaborators, Calco Chemical Company, Bound Brook, N. J.

"Pharmacological Studies," by B. K. Harned and collaborators, Lederle Laboratories, Pearl River, N. Y.

"Physiological Aspects," by F. S. Daft, National Institute of Health, Bethesda, Maryland.

"Vitamin M Deficiency," by John R. Totter, University of Arkansas, School of Medicine, Fayetteville, Arkansas.

"Clinical Aspects," by T. D. Spies, Hillman Hospital, Birmingham, Alabama.